



## Theoretical Issues in Economic Anthropology

George Dalton

*Current Anthropology*, Vol. 10, No. 1. (Feb., 1969), pp. 63-102.

Stable URL:

<http://links.jstor.org/sici?sici=0011-3204%28196902%2910%3A1%3C63%3ATIIEA%3E2.0.CO%3B2-F>

*Current Anthropology* is currently published by The University of Chicago Press.

---

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/about/terms.html>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Please contact the publisher regarding any further use of this work. Publisher contact information may be obtained at <http://www.jstor.org/journals/ucpress.html>.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

---

JSTOR is an independent not-for-profit organization dedicated to and preserving a digital archive of scholarly journals. For more information regarding JSTOR, please contact [support@jstor.org](mailto:support@jstor.org).

# Theoretical Issues in Economic Anthropology<sup>1</sup>

by George Dalton

THE PUBLICATION of *Trade and Market in the Early Empires* (1957), and other work associated with Karl Polanyi's analytical schema, has intensified theoretical disputes in economic anthropology having their origins much earlier when the subject was of interest to many fewer anthropologists (Firth 1939: Chap. 1; Herskovits 1940: Chap. 2; 1941). These disputes are principally over which of several alternative sets of analytical concepts are best to interpret real world processes and institutions, and what kinds of analytical questions should be put to primitive and peasant economies—those asked by economists about our own economy, or questions having to do with the connections between economic and social organization.

This paper is addressed to issues raised recently by anthropologists who have criticized the theoretical approach of Polanyi and myself (LeClair 1962; Burling 1962; Pospisil 1963; Cook 1966). Part I states the positions of Polanyi and his critics and explains why controversy per-

sists; Part II suggests a theoretical framework for economic anthropology which takes account of both sets of ideas. Part III considers the recent extension of economic anthropology to processes of socio-economic change, growth, and development in communities undergoing "modernization".

## I. THE CONTROVERSY

Whether one is collecting and analyzing fieldwork information (e.g., Salisbury 1962) or writing a general or comparative work drawing on the extensive ethnographic literature (Herskovits 1952, Belshaw 1965, Nash 1966), one must choose a theoretical approach. Both fieldwork and synthesis in economic anthropology suffer from inadequate theory.

(1) Anthropology has devoted less attention to economic organization and performance than to, e.g., kinship or politics. Only two anthropologists, Bronislaw Malinowski and Raymond Firth, have a large corpus of writings in economic anthropology and relatively few—principally Cyril Belshaw, Paul Bohannan, Mary Douglas, Clifford Geertz, Maurice Godelier, Melville Herskovits, Sidney Mintz, Manning Nash, Marshall Sahlins, and Richard Salisbury—have written at length on its theoretical aspects.<sup>2</sup>

(2) Anthropologists understand the economic organization and the economic theory of industrial capitalism much less well than they understand European and American politics, kinship, and religion and the theories which explain them. Some anthropologists seem not to understand that conventional economics—the most abstract and mathematical of the social sciences—does not

---

GEORGE DALTON holds a joint appointment as a Professor in the Departments of Economics and Anthropology at Northwestern University and is a staff member of its Program of African Studies. He was born in 1926. He received his B.A. (Economics) from Indiana University, Bloomington, Ind., U.S.A., M.A. (Economics) from Columbia University, New York, N.Y., and his Ph.D. (Economics) from the University of Oregon, Eugene, Ore. He has taught at Boston University, Mass. and Bard College. In 1961-62 he spent 15 months in West Africa as Deputy Director of Northwestern University's Economic Survey of Liberia, and is co-author of *Growth Without Development: An Economic Survey of Liberia* (Clower, Dalton, Harwitz, and Walters 1966). With Paul Bohannan, he edited *Markets in Africa* (1962; abridged edition, 1965). He has published articles on economic anthropology, economic development, and comparative economic systems, and has edited a book of readings in economic anthropology, *Tribal and Peasant Economies* (1967), and a collection of the essays of Karl Polanyi, *Primitive, Archaic, and Modern Economies* (1968a). In 1967-68 he was President of the Association for Comparative Economics. He is currently working in collaboration with Irma Adelman on a statistical analysis of social and economic development of village communities in India.

The present article, submitted for publication 26 vi 67, was sent for CA ☆ treatment to 50 scholars, of whom the following responded with written comments: Conrad M. Arensberg, John Blacking, Helen Codere, Erik Cohen, Peter van Emst, Ronald Frankenberg, Morris Freilich, G. P. Grigor'ev, Zbigniew Marcia Kowaleski, Edward E. LeClair, Jr., Lorenz G. Löffler, Simon D. Messing, P. K. Misra, Manning Nash, Roman Raczynski, Joyce F. Riegelhaupt, Richard F. Salisbury, H. K. Schneider, D. P. Sinha, Arnold Strickon, Carol F. Swartout, Louise E. Sweet, Stanislaw Tabaczynski, and Andrew P. Vayda. Their comments are printed in full after the text and are followed by a reply from the author.

---

<sup>1</sup> I have been very fortunate in getting critical comments on earlier drafts of this paper from anthropologists, economists, and others whom it is a pleasure to thank here: Irma Adelman, Conrad Arensberg, Joseph Berliner, Paul Bohannan, David Brokensha, Heyward Ehrlich, Everett Hagen, Thomas Harding, Clifford Geertz, Peter McLoughlin, Sidney Mintz, Walter Neale, and Ilona Polanyi.

<sup>2</sup> I do not mean to slight the contributions of others, living or dead, (e.g., Mauss 1954) but only to name those who have written at some length on the theoretical aspects of economic anthropology. See Godelier (1965), and Dalton (1967) for further references to theoretical writings.

deal with what anthropologists mean by human behavior, and that the concepts of conventional economics relating to economic *organization* are not (with some minor qualifications) fruitfully applicable outside of market systems. What is a serious point of contention among anthropologists (LeClair 1962, Burling 1962, Cook 1966) is dismissed out of hand by a prominent development economist (Lewis 1962:viii):

... the economist who studies the non-market economy has to abandon most of what he has learnt, and adopts the techniques of the anthropologist.

(3) Economists have not been concerned with primitive and peasant societies and economies. Until recently they had been concerned exclusively with one type of economy, industrial capitalism. The concepts, leading ideas, and causal analyses of price theory, aggregate income theory, and growth theory—as well as fields such as international trade, and money—deal with the structure, performance, and problems of U.S. and West European economies in the 19th and 20th centuries. Economists have had no reason to spell out the content and method of economics in such fashion as to make clear what in conventional economics is and what is not relevant to economic anthropology.

The fact that the attention of economists has been focused so exclusively on just those aspects of our economy least likely to be found among non-literate folk has thus confused anthropologists who turned to economic treatises for clarification of problems and methods in the study of the economic systems of non-literate societies (Herskovits 1952:53).

Very few economists other than myself and those represented in *Trade and Market* (Polanyi, Neale, Pearson, and Fusfeld) have written on these issues. The few economists who have written on the relevance of economics to economic anthropology have done so principally in book reviews (Knight 1941; Rottenberg 1958; Boulding 1957).

The field of comparative economic systems came into being only in the 1930's and '40's (with central planning in the Soviet Union, enlarged governmental spending, borrowing, taxing, and market controls in fascist Germany, the American New Deal, and welfare state reforms in England and Scandinavia). The field remains confined to the study of national industrialized economies. It compares the structure and performance of U.S. and Soviet type economies, and considers the literature of socialism (Gruchy 1966). Comparative economic systems has never included the economies studied by anthropologists nor those described in the literature of pre-industrial economic history of Europe.

The field of economic development, which came into being after World War II with the achievement of political independence by former colonies in Africa and Asia, brought American and European economists for the first time into those areas of the world in which anthropologists have traditionally centered their interests. Most of the literature of economic development deals, however, with the same impersonal matters of investment and foreign trade relating to the *national* economy (e.g., Nigeria, not the Tiv) that economists are concerned with in analyzing our own national economy. A small part of the development literature written by economists is concerned with

social organization and culture (what economists call "institutional matters"), as these relate to national and local community development (Lewis 1955; Myrdal 1957; Hagen 1962; Yudelman 1964; Adelman and Morris 1965, 1967; Dalton 1965c).

With the minor and recent qualifications of comparative economic systems and economic development, conventional economics excludes entirely from its formal analyses matters relating to social organization and other aspects of culture. Thus the relation of economic to social organization—a problem important in economic anthropology—does not arise in conventional economic theory. It is unfortunate that anthropologists have turned for guidance to post-industrial economic theory rather than to pre-industrial European economic and social history. Pirenne (1936), Bloch (1961, 1966), Bennett (1962), Weber (1950), and Polanyi (who was an economic historian) have more to teach anthropologists about the economies they study than do Marshall (1920), Knight (1941), or Robbins (1935).

(4) Almost all the communities anthropologists study in the field are now experiencing some degree of economic, social, cultural, or technological change as parts of newly independent nation-states bent on "modernization" and economic development. The subject of socio-economic change within the context of developing nation-states is extremely complicated and, of course, very recent. We know more about "traditional" systems before Western incursion and about the kinds of change that took place in the 19th century with colonialism. The scope of economic anthropology is now widening considerably. The older focus of interest was the organization and functioning of indigenous economy as it relates to social relationships at one point in time, or under conditions of slow change (Malinowski 1922; 1935). The new focus of interest is modernization. The newly established political independence of Asian and African nation-states and their governments' explicit intention to create and develop national societies and economies makes the context of present-day community change sufficiently different from that of the early culture contact studies (e.g., on the depopulation of Melanesia or the introduction of the horse among the Plains Indians) to require new theoretical approaches and new policy concerns.

In the absence of adequate theory, controversy persists as to the merits of various alternative frames of reference for the analytical treatment of primitive and peasant societies. The main point of contention has aroused heated controversy: the extent to which anthropologists should adopt conventional economics as the conceptual language with which to analyze primitive and peasant economies (Firth 1958:63). Unlike Auden's academic warriors who "fight with smiles and Christian names", some of the participants in this dispute display the ferocity of those engaged in theological battle, a battle which has now become a thirty years war (Goodfellow 1939: Chap. 1; Firth 1939: Chap. 1; Herskovits 1940: Chap. 2, 1941, 1952; Knight 1941; Polanyi 1944: Chap. 4, 1947, 1957; Rottenberg 1958; Sahllins 1960; Dalton 1961, 1962, 1965a; LeClair 1962; Burling 1962; Cook 1966).

The economic anthropologist finds two ready-made bodies of economic theory—concepts, leading ideas, terminology, and generalizations—both created to analyze industrial capitalist economies: conventional economic

theory and Marxian theory.<sup>3</sup> The question that confronts him is whether to borrow concepts and leading ideas from these, or to invent a special set of concepts and leading ideas having no counterparts in conventional and Marxian economics—or, indeed, to use some combination of conventional economics, Marxian economics, and a special set of concepts designed for primitive and peasant economies.

The problem seems no longer to arise in other branches of anthropological inquiry. Anthropologists seem to agree that it is inappropriate to borrow concepts and leading ideas from Western religious theory (Christianity and Judaism), and political theory (democracy and dictatorship) to analyze religious and political organization:

Nothing is so misleading in ethnographic accounts as the description of facts of native civilizations in terms of our own (Malinowski 1922:176).

The mistake of judging the men of other periods by the morality of our own day has its parallel in the mistake of supposing that every wheel and bolt in the modern social machine has its counterpart in more rudimentary societies (Maine, quoted in Bohannan 1957:iii).

. . . most anthropologists have ceased to take their bearings in the study of religion from any religion practiced in their own society (Leinhardt 1956:310).

One important discovery made in . . . [*African Political Systems*] was that the institutions through which a society organized politically need not necessarily look like the kinds of political institutions with which we have long been familiar in the Western world, and in the great nations of Asia (Gluckman and Cunison 1962:vi).

In economic anthropology, however, there are those who argue that the leading ideas, concepts, and terminology of conventional economic theory (economizing, maximizing, elasticity, scarcity, supply, demand, capital, etc.) are applicable to primitive as well as peasant economies studied by anthropologists; that the basic similarities between primitive and peasant economies and industrial capitalism are sufficiently close so that some sort of universal economic theory—embracing the very large number of economies studied by anthropologists as well as our own—is achievable; and that anthropologists should learn more conventional economics so as to be able to put the same questions about economic *performance* to their data that economists put to theirs, and (as economists do) to quantify their data when possible (Firth 1957, 1964, 1965; Salisbury 1962: Chaps. 6–9).

With a few exceptions (Bohannan and Dalton 1965; Dalton 1964) Polanyi and the group associated with him have not concerned themselves with *peasant* economies. My own view is that conventional economics is relevant to the commercialized sectors of peasant economic *organization* (i.e., where dependence on purchased land, wage-labor, and the market sale of produce is quantitatively important), and useful in quantifying economic *performance*—the amounts and composition of produce—for any economy, primitive, peasant, industrial capitalist, or industrial com-

munist. But that the differences between primitive economic organization (i.e., where market transactions of resources and produce are absent or present only in petty amounts) and our own are so great that a special set of concepts, leading ideas, and terms are necessary to analyze these subsistence economies. Special analytical concepts are necessary because social organization and culture—kinship, political organization, religion—affect economic organization and performance so directly and sensitively in non-market systems that only a socio-economic approach which considers explicitly the relationships between economy and society is capable of yielding insights and generalizations of importance (Sahlins 1968: Chap. 5). A special set of questions should be put to primitive economies and the non-commercial sectors of peasant economies: questions about the social aspects of economic organization.

The ties between producers tend to reach out beyond this common interest in the act of production and its rewards alone. A production relationship is often only one facet of a social relationship . . . Economic relations can be understood only as a part of a scheme of social relations . . . Economic anthropology deals primarily with the economic aspects of the social relations of persons (Firth 1951:136–38).

Polanyi focuses on economy as a set of rules of social organization and on socio-economic structure (organization) rather than quantifiable performance (levels of output; productivity). Moreover, he confines his analysis to primitive and archaic (state-organized, pre-industrial) economies.

The two groups agree that knowledge of economics and of our own economic system, industrial capitalism, should figure explicitly and importantly in economic anthropology. They disagree sharply, however, on the appropriate way to incorporate conventional economics and knowledge of our own economy. The “formalist” group takes what it believes to be the universally applicable concepts of economic theory—scarcity, maximizing, surplus—as that which is to be incorporated in economic anthropology and analyzes the empirical data of primitive and peasant economies in these terms (Pospisil 1963; Firth 1965). They use the leading ideas of elementary economics (Samuelson 1967: Chaps. 1–3) as a guide to analyzing all economies.

Polanyi (1944: Chap. 4), Neale (1957a), Fusfeld (1957), and I (Dalton 1961; 1962) describe the salient characteristics of industrial capitalism and economic theory so as to provide a base of contrast with that sub-set of economies we call “primitive.” We then show how variants of reciprocity and redistribution act as integrating organizational principles of land and labour allocation, work organization, and produce disposition (Polanyi 1957; Neale 1957), and how external trade and money uses in such economies are derivative expressions of reciprocal and redistributive modes of transaction (Bohannan 1959; Dalton 1965). Polanyi argues that the concepts of economic theory yield useful insights when applied to our own economy because the institutionalized rules of market exchange and the use of our kind of money and technology induce economizing and maximizing activities; but to employ these terms to analyze the non-market sectors of primitive and peasant economies is as distorting as it

<sup>3</sup> Among those anthropologists who write in English there is only an occasional borrowing of Marxian concepts (such as economic surplus), rather than a systematic attempt to apply Marxian analysis to primitive and peasant economies (Herskovits 1952: Part V; Pearson 1957; Harris 1959; Dalton 1960, 1963).

would be to use the concepts of Christianity to analyze primitive religions.

The "formalist" group confuses the *ability* to translate any socio-economic transaction or exchange (potlatch, kula, bridewealth) into market terms with the *usefulness* of doing so (Homans 1958; Pospisil 1963). Describing the potlatch as an investment which yields 100 per cent interest (Boas 1897), bridewealth as the price one pays for sexual and domestic services (Gray 1960), and shell transactions on Rossel Island as cash payments for market purchases (Armstrong 1924; 1928), suggests they are basically similar to ordinary commercial transactions in our own economy. These are the analytical views of the anthropologist, his *interpretation* of real world processes which remain the same regardless of what he calls them. Whether it is analytically revealing to interpret the potlatch, bridewealth, and Rossel Island shells in such fashion depends on the folkviews of these events and usages, as well as on an assessment of the differences between these and commercial transactions (Dalton 1961: 10-14; 1966).

#### THE QUEST FOR A UNIVERSAL THEORY

There is a deep-seated yearning in social science to discover one general approach, one general law valid for all time and all climes. But these primitive attitudes must be outgrown (Gershenkron 1954:256).

Those who insist on the applicability of conventional economics to primitive economies are really in quest of a universal theory—a single set of concepts which would yield fruitful insights for all economies, those studied by anthropologists as well as those studied by economists and historians.<sup>4</sup>

What is required . . . is a search for the general theory of economic process and structure of which contemporary economic theory is but a special case (LeClair 1962:1188).

What is required from economic anthropology is the analysis of material in such a way that it will be directly comparable with the material of modern economics, matching assumption with assumption and so allowing generalizations to be ultimately framed which will subsume the phenomena of both price and non-price communities into a body of principles about human behavior which will be truly universal (Firth 1966:14).

The difficulty with this approach is that economic anthropology deals with an extraordinary range of matters in an extraordinarily large set of economies: it is concerned with *structure and performance of both primitive and peasant economies under static and dynamic conditions*. What Firth and LeClair suggest be treated under a single theory is assigned in economics (and sociology) to several sub-

<sup>4</sup> Clifford Geertz quite rightly points out (in private correspondence) that people confuse the generality of a theory with its ability to be applied universally. Polanyi most certainly is not arguing against generalization or abstract conceptualization; rather, he argues against the position that conventional economics—designed to analyze nationally-integrated, industrial, market economies—provides an adequate conceptual basis for a universally applicable theory of economic structure and process. Specifically, he shows that for economies lacking the salient organizational features of developed capitalism (market integration, machine technology, and the modern kind of money—primitive and archaic economies, in his terms), principles of socio-economic organization exist which require for their analysis conceptual categories different from those of conventional economics (Polanyi 1957; Dalton 1961).

fields: price and distribution theory; aggregate income theory; growth theory; comparative economic systems; national income accounting; industrial sociology, etc. In economics we have one set of analytical concepts to answer the question, "What determines prices in U.S. type economies?" And we have a different set to answer the question, "What determines gross national product?"—and yet another to answer the question, "How do we measure national income?" In economic anthropology we also need several sets of analytical and measurement concepts because of the several kinds of economies we are dealing with and the different kinds of questions we put to the data.

In summary, those of the thousands of economies studied by anthropologists that are underdeveloped, small-scale market systems—peasant economies (Tax 1963; Firth 1946) can be fruitfully analyzed with the concepts of conventional economics. Moreover, the quantifiable performance of all economies, primitive and peasant included, can be measured in simplified terms analogous to national income accounting terms. But to put interesting questions about the organization of traditional, primitive economies, and primitive and peasant economies undergoing change, growth, and development, requires conceptual categories different from those used in conventional economics.

#### THE ECONOMY: INDIVIDUAL BEHAVIOR VS. RULES OF SOCIAL ORGANIZATION

Another underlying difficulty is the existence of two rather different ways of perceiving an economy: one is to concentrate on economic "behavior" of individual persons and the motives that impel the individual behavers, so that the economy is seen as a cluster of individual actors and their motives.

Economics . . . has long been confidently felt to include a tolerably well-defined type of human behavior . . . None of these definitions [of economics] covers exactly the same area of behavior as any other. . . . Even economists have long claimed to equate the material side of life with economic behavior. . . . Since this definition does not isolate any type of behavior from any other type. . . . If the unity of economics arises out of the fact that it deals with priced goods, then in some primitive societies it is silly to look for any behavior that can be called "economic". . . . Economics in this view focuses on a particular *aspect* of behavior and not on certain kinds of behavior (Burling 1962:802, 805, 808, 811).

The other approach is to perceive the economy as a set of rules of social organization (analogous to polity and political rules), so that each of us is born into a "system" whose rules we learn. It is from observing the activities and transactions of participants that we derive these systematic rules. This is how Polanyi regards an economy, and, indeed, it is the approach used in comparative economic systems in contrasting the organization (rules) of Soviet and U.S. economies.

Those who perceive an economy as a cluster of individual behavers frequently equate whatever economic activities the behavers undertake with explicit *choice* of those activities, and believe that such choice affirms the economics textbook dictum that in all economies there must be choice of what to produce, how to produce it, and who is to get how much of what is produced. This way of intro-

ducing the topics of resource allocation, production functions, and income distribution in industrial capitalism to beginning students in economics is useful because the individual households and firms in national market economies such as our own are confronted with many explicit choices: which of thousands of goods and services to buy; which of hundreds of job markets to enter; which of dozens of products to produce; which of several techniques to use to produce them. These alternative choices are subject to fine calculation because industrial capitalism makes extensive use of money and pricing, and because there are real alternatives among which economic choices can be made without calling down social opprobrium. For example, the American farmer, entirely depending on market sale for livelihood, must choose explicitly how much of each kind of cash crop to grow. The relevant considerations are not personal taste, social obligation, or physical yield, but physical yield times expected money price compared to money costs of production. He makes explicit "economizing" decisions about costs relative to expected market revenue for the several alternative crops he can grow, and the several alternative combinations of resources he can buy to grow them. His livelihood depends on such choices.

In subsistence (non-market) economies, the question of choice among real alternatives does not arise in such explicit fashion. A Trobriand Islander learns and follows the rules of *economy* in his society almost like an American learns and follows the rules of *language* in his. An American is born into an English-speaking culture. In no sense does he "choose" to speak English because no real alternative is presented to him. So too, the Trobriander is born into a yam-growing economy. He does not "choose" to plant yams rather than broccoli. The question does not arise in this form, but rather in the form of how much of each of very few conventional crops to plant or how to apportion a given work day to several tasks.

In the Trobriand subsistence economy, labor, land and other resources are not purchased, and produce is not destined for sale to others, so it is personal taste within the *ecological* constraints set by resource endowment, the *technological* constraints set by known techniques of production, and the *social* constraints set by the obligation to provide sister's husband with yams that dictate how much of each crop is to be planted.<sup>5</sup> So too for Tikopia:

... in Tikopia on any given day a man has in theory a choice between working in his orchard and going out fishing, in a canoe or on the reef. It might be held that he will decide according to his preference at the time for an ultimate yield of crops or an immediate one of fish. But in practice his choice may be rigidly determined by social and ritual considerations. The recent death of a man of rank and the taboos associated with mourning may bar him absolutely from any resort to canoe-fishing out at sea, although such may otherwise be his preference and would yield him a greater material return. . . . Moreover, the period of his abstention from canoe-fishing tends to vary directly in accordance with his propinquity of kinship to the dead (Firth 1966:12).

In primitive economies, the constraints on individual choice of material goods and economic activities are ex-

treme, and are dictated not only by social obligation but also by primitive technology and by physical environment. There is simply no equivalent to the range of choice of goods and activities in industrial capitalism which makes meaningful such economic concepts as "maximizing" and "economizing." Nor is there the possibility of fine calculation in monetary terms which pricing allows.

As the literature of theoretical contention in economic anthropology grows, it becomes increasingly clear that those who argue that conventional economics is *applicable* to primitive economies—the Trobriand and Tiv type of economy—have three things in mind:

(1) The first is the least difficult to unravel. It is to regard peasant economies as the typical cases to be analyzed in economic anthropology and to assume that what is true for peasant economies is also true for primitive economies because they are both within the universe of anthropological interest, and somehow a single set of concepts and generalizations should apply. Peasant economies are small-scale, underdeveloped market economies, in which production for market sale, the use of Western money, the availability of purchased factors of production, and other features of market economies are present. The structure and performance of the commercialized sectors of peasant economies are amenable to analysis and measurement in conventional economic terms (precisely because money, prices, and markets are important). But this does not mean that the same is true for primitive economies—the Trobriands, the Nuer—in which the crucial features of market organization which allow analysis in market concepts and measurement in money terms, are absent.

(2) The second reason some anthropologists think conventional economics is applicable to all economies is—to speak bluntly—due to their imperfect understanding of economic theory and its concepts. From Goodfellow (1939) and Hershkovits (1952), to Burling (1962) and Cook (1966), there is misunderstanding of what economists mean by "scarcity," "economize," "maximize," and "rational choice." Given cost and demand schedules for a firm, there is one price-output combination at which a firm maximizes its profit. This can be shown unambiguously by reference to other price-output combinations for the firm. Anthropologists misuse—or better, mis-translate—this piece of analysis by erroneously equating all *purposeful* activities with economizing or maximizing, and then jumping to the conclusion that because purposeful choices are made in primitive economies, economic theory must apply.

Our primary concern in these pages is to understand the cross-cultural implications of the process of economizing (Hershkovits 1952:4).

From this point of view, we are "economizing" in everything we do. We are always trying to maximize our satisfactions somehow, and so we are led back to the notion that economics deals not with a type but rather with an aspect of behavior. This economic view of society becomes . . . one model for looking at society. It is a model which sees the individuals of a society busily engaged in maximizing their own satisfactions—desire for power, sex, food, independence . . . (Burling 1962:817–18).

Such misinterpretations of economic concepts persist because economists who have had occasion to deal with anthropology or primitive societies—Irma Adelman,

<sup>5</sup> Just as an American who somehow chose not to learn English would be severely penalized by the social system in finding jobs, in his ability to communicate with others, etc., so too would a Trobriander be penalized in his society by choosing not to grow yams.

Joseph Berliner, Everett Hagen, Arthur Lewis—have not addressed themselves to these matters, and the economists who have are associated with Polanyi. Because a Tikopian chooses to fish today rather than to tend his garden does not mean that the economics of Tikopian fishing or gardening is usefully described by linear programming or oligopoly theory.

(3) The third way in which it is thought that conventional economics applies to primitive economy is in the measurement of economic performance. Several branches of applied economics and statistics measure production flows (e.g., input-output analysis), and total output and its composition (e.g., national income accounting), for large-scale, nationally-integrated, industrialized economies of both the U.S. and Soviet types. Some kind of measurement of output is possible for any type of economy, no matter how primitive and small, because it is always possible to measure output and performance in terms of the resources used (labor days to build a hut), or in the real terms of the produce forthcoming (tons of yams produced).

Salisbury's book on a primitive economy in New Guinea before and after the introduction of purchased steel axes is cited by Firth (1965) and Cook (1966) as proof of the ability to "apply" conventional economics to a primitive economy because Salisbury does some rather elementary calculations such as the number of man-days of labor required to produce a variety of items (1962:147). Such calculations can be done for any economy—Robinson Crusoe's, a medieval monastery, an Israeli kibbutz, or Communist China.

If what anthropologists mean by "applying economic theory" is to count the number of yams produced and the number of labour days needed to build a hut, then most certainly economic theory is applicable to all economies. But this is a rather simplistic notion of what "applying economic theory" means.

A prominent national income economist at Cambridge says the following about her experience in measuring subsistence income in rural Rhodesia:

An attempt to examine the structure and problems of a primitive community in the light of the existing body of economic thought raises fundamental conceptual issues. Economic analysis and its framework of generalizations are characteristically described in terms appropriate to the modern exchange economy. It is by no means certain that the existing tools of analysis can usefully be applied to material other than that for which they have been developed. In particular it is not clear what light, if any, is thrown on subsistence economies by a science which seems to regard the use of money and specialization of labor as axiomatic. The jargon of the market place seems remote, on the face of it, from the problems of an African village where most individuals spend the greater part of their lives in satisfying their own or their families' needs and desires, where money and trade play a subordinate role in motivating productive activity (Deane 1953:115-16).

It is true that in attempting to measure economic performance quantitatively, anthropologists put the same questions to small subsistence economies that economists put to our own and the Soviet national economy: What is the total output and its composition for the community? How is income divided? But the absence of cash and pricing means that only crude estimates of output can be indicated—nothing like the detailed components of national income and gross national product for developed economies; and the small number of goods and services

produced together with the absence of complicated processes of manufacture and fabrication (the absence of inter-firm and inter-industry transactions in developed economies) means that input-output analysis yields no useful information.

I would agree emphatically that output statistics for subsistence economies are worth having especially in the analysis of community change, growth, and development (Epstein 1962). They would give us additional information, along with knowledge of socio-economic organization, of the pre-modernization economy, as well as rough benchmarks from which to measure growth. I suggest, however, that for traditional, slow-changing, subsistence (and peasant) economies, it is the analysis of socio-economic organization rather than performance that yields insights of comparable interest and depth to those got in analyzing primitive religions, polity, kinship, etc.

#### TRADITIONAL, SUBSISTENCE ECONOMIES

Cook (1966) criticizes Polanyi and myself for analyzing in detail the structure of traditional subsistence economies such as the Trobriand Islands in Malinowski's time. In the mid-1960's very few such economies exist intact, almost all of them undergoing various kinds and degrees of economic, social, cultural, and technological change.<sup>6</sup> This kind of complaint seems not to be made in other branches of anthropology: do anthropologists criticize each other for studying traditional political organization or traditional religion because—like traditional economy—they are now undergoing change? Indeed, why study history, then, since it is concerned with forms of social organization no longer in being?

Here we have an example of an odd double standard in anthropology. Anthropologists who would condemn out of hand a theoretical approach which regarded primitive religion or political organization as being simply variants of European religions and polities to be analyzed in the conceptual language of Christianity and democracy, nevertheless approach primitive economy as though it were simply a variant of capitalism to be analyzed in the conceptual language of supply, demand, elasticity, capital, maximizing, (bride) price, etc. (LeClair 1962; Pospisil 1963).

To answer the question specifically: Anthropologists have both old and new reasons to study the organization of traditional, subsistence economies, even in the 1960's when these are changing. One old reason is precisely the same that justifies their studying other aspects of traditional social organization and culture—religion, polity, kinship, language: to find out how these are (or were) organized in as many societies as we can, make analytical generalizations about them, and compare them to our own Western systems. But there are special reasons as well.

<sup>6</sup> Although, in the mid-1960's, there are very few pure subsistence economies, (economies in which commercial transactions are entirely absent), there are a good many primitive economies (especially in Africa) in which half or more of total income comes from subsistence production, and peasant economies in which smaller, but significant amounts of subsistence production are the rule. In the early 1950's, a UN agency reported that "... between 65 per cent and 75 per cent of the total cultivated land area of tropical Africa is devoted to subsistence production" (United Nations 1954:13). In the 1960's, the problem of transforming subsistence agriculture in Africa is still very much a matter of concern. See Yudelman (1964); Clower, Dalton, Harwitz, and Walters (1966); Adelman and Morris (1967).

Writings in economic anthropology are either descriptive ethnographies or theoretical analyses. The theoretical portion of economic anthropology has been poorly done. I do not mean to suggest that theoretical light began to dawn only with the publication of *Trade and Market in the Early Empires*. Malinowski (1921; 1922; 1935), Mauss (1954), and Firth (1929; 1939; 1946)—to name only the outstanding—have made contributions of great importance. But much was not done, and much of what was done was done poorly. And it is Polanyi's work on modes of transaction, money, markets, external trade, and operational devices in primitive and archaic economies that has begun important new lines of analysis, and, indeed, has allowed us to clear up some old muddles such as "primitive money" (Polanyi 1957; 1968; Dalton 1965), and economic "surplus" (Pearson 1957; Dalton 1960; 1963).

The peoples and communities of Africa, Asia, Latin America, and Oceania traditionally studied by anthropologists are experiencing the several kinds of change entailed in economic development, industrialization, urbanization, and the formation of nation-states. Anthropologists are increasingly concerned with the processes and problems of socio-economic change. There is a rapidly growing literature of theory and case studies (Smelser 1963; Douglas 1965; Brokensha 1966). Indeed, anthropologists have returned to places they did fieldwork in 20 or more years earlier, to study socio-economic change (Firth 1959; 1966).

I suggest that analytical insights and generalizations about change and development have to be based on firm understanding of traditional socio-economic organization (Dalton 1964; 1965*b*). Change is always change of what is; and what is, depends on what has been: "Any planned growth is embedded in a set of institutions and attitudes which come from the past" (Keyfitz 1959:34).

One can illustrate the point from European and American experience. How is it possible to understand the causes and consequences of those New Deal, Fair Deal, or "Great Society" changes in the U.S. economy and their counterparts in the English and Scandinavian welfare states, except by knowing the structure and performance of 19th and early 20th-century capitalism in Europe and the U.S.? How is it possible to understand the impact of Western money on subsistence economies in Africa unless one first understands the nature of indigenous money and its uses, which, in turn, requires knowing how indigenous economy functioned before the monetary incursion (Bohannan 1959; Douglas 1958)? So too, in order to understand why litigation over land rights sometimes occurs when land is first made subject to contractual purchase and sale, one has to know the nature of land tenure before land was made marketable (Biebuyck 1963).

Processes of modernization—industrialization, the expansion of commercial production—ramify into all segments of society and culture. Many of the anthropological studies being undertaken are addressed to two broad questions, both of which require knowledge of traditional, "pre-modernization" structures: (1) What are those features of traditional social organization, culture, polity, and economy which make for receptivity or resistance to technological, economic, and cultural innovations (Douglas 1965)? (2) What are the "impacts"—processes of sequential change—on traditional social organization and culture when a group undertakes enlarged production for

sale, the use of Western money and technology, and incorporates other such innovations (Gulliver 1965; Epstein 1962; Firth and Yamey 1964)?

## II. A THEORETICAL FRAMEWORK FOR ECONOMIC ANTHROPOLOGY

A good theoretical framework for economic anthropology should be clear about the similarities and differences between our own economy and primitive and peasant economies, about the relevance of conventional economics to economic anthropology, and it should contain an explicit statement of the matters to be analyzed, economic anthropology, for example:

TABLE 1

1. Socio-Economic Structure:	Primitive Economies, before modernization Peasant Economies, before modernization
2. Economic Performance:	Primitive Economies, before modernization Peasant Economies, before modernization
3. Socio-economic Organization and Economic Performance in Primitive and Peasant Economies Compared to Industrial Capitalism.	
4. Processes and Problems of Socio-Economic Change, Growth, and Development in Primitive and Peasant Communities.	

I shall discuss some of the conceptual categories I think most useful in economic anthropology, indicate the questions they help answer, and the leading ideas they are associated with (see Tables 2 and 3, pp. 70, 71). In doing so I hope to make several points: to show how much at the beginnings of theoretical analysis we are in economic anthropology; to show what a wide variety of structures, processes and problems are dealt with in the subject; and to suggest lines of analysis and conceptual categories that seem promising.

### ECONOMIC ANTHROPOLOGY AS PART OF COMPARATIVE ECONOMY

The economies of direct interest to anthropologists are the large set of subsistence and peasant communities in Africa, Asia, Latin America, Oceania, and the Middle East. The focus of analytical interest is either their traditional structure and performance before serious Western incursion (Malinowski 1922; 1935), or matters relating to socio-economic change and development (Epstein 1962; Firth 1966). In either case there is an important literature outside of anthropology. The fields within economics which provide complementary information are pre-industrial economic history (Postan 1966; Takizawa 1927), comparative economic systems (Grossman 1967; Myrdal 1960; Carr 1951), and the institutional literature of economic development (Lewis 1955; Myrdal 1957; Hagen 1962; Adelman and Morris 1967).

Economic anthropology is best done within a framework of comparative economic systems which draws on all economies of record. The analysis of pre-industrial, developed, and developing economies is now scattered in



TABLE 2  
ECONOMIES OF RECORD AND SOCIAL SCIENCE SUB-FIELDS

*Economies of Record*

<i>Small-scale</i>			<i>National</i>		
<i>Primitive and Peasant, Before Modernization</i>	<i>Primitive and Peasant, Change and Development</i>	<i>Utopian<sup>a</sup></i>	<i>19th-Century Capitalism</i>	<i>Welfare State and Fascism</i>	<i>Communist</i>
Economic Anthropology	Economic Anthropology Applied Anthropology	European and American History	Economic History	Comparative Economic Systems	Soviet Economy <sup>b</sup>
Pre-industrial Economic and Social History (e.g., Europe and Asia)	Economic Development		History of Economic Thought	Economic History	Comparative Economic Systems
	Economic History		Classical and Neoclassical Economic Theory	Modern Economic Theory	
			Industrial Sociology	Industrial Sociology	Industrial Sociology

<sup>a</sup> The important connections between the structure of traditional, primitive economies and utopian communities (Noyes 1870; Nordhoff 1961; Bestor 1950; Bishop 1950) have never been systematically analyzed. Both kinds are small-scale economies whose internal organization is of non-market sorts; where production processes—especially land tenure, work organization, and produce allocation—express social relationships. It is this feature which makes writers like Nyerere (1964) and Senghor (1964) assert that traditional African communities had a “socialist” ethos.

<sup>b</sup> Soviet economy has developed as a separate field of specialization within economics; see Nove (1962).

various branches of economics, history, sociology, and anthropology, all of which contribute information of use to the broad range of topics considered in economic anthropology (see Table 2).

WHAT IS AN ECONOMIC “SYSTEM”?

One of the many semantic difficulties in economic anthropology is that the word “economy” (like the words “society” and “culture”) has no size dimension attached to it. We can speak of the economy of a hunting band comprising a few dozen persons or the economy of Communist China comprising several hundred million.

Whatever the size of the economy it will have several features in common, three of which are of special interest.

(1) Whether the human group is called band, tribe, village, or nation, and whether its economy is called primitive, peasant, capitalist, or communist, it consists of people with recognized social and cultural affinities—kinship, religion, language, neighborhood—expressed in some sort of shared community or social life. This means that two kinds of goods and specialist services<sup>7</sup> must be

<sup>7</sup> The concept of “services” causes difficulty in economic anthropology (as do the concepts of “capital” and “market”) because the term is used to cover a wide range of items or activities in our own economy, only a few of which are found in primitive economies. In our own economy, the term “services” is used to describe ordinary labor, mechanized utilities (telephone and electricity services), the services performed by craftsmen and professional specialists, e.g., dentistry, TV repairs, musicians; and also the functions performed by political and religious office-holders. In our own economy, all but the latter services are organized for purchase and sale. In relation to primitive and peasant economies, I prefer to use the term “specialist services” to refer to those provided by craftsmen, such as blacksmiths, woodcarvers, and dancers, and those provided by persons performing political, religious, and ritual roles.

provided for use within the community (however defined): food and other material requisites of *physical* existence, and goods and services for religion, defense, settlement of dispute, rites of passage, and other aspects of *social* and community life. The acquisition or production of material items and specialist services necessary for physical and social existence are never left to chance because neither individuals nor communities can survive without them. It is for this reason that it is useful to regard all communities or societies as having economic systems. The word “system” refers to structured arrangements and rules which assure that material goods and specialist services are provided in repetitive fashion. One task of economic anthropology is to spell out these rules and systematic arrangements for that set of societies of interest to anthropologists.

(2) A second similarity among economies is that they all make use of some form(s) of natural resources (land, waterways, minerals), human co-operation (division of labor), and technology (tools, and knowledge of production or acquisition processes). Each of these features is structured: the use of tools, natural resources, and division of labor require social rules—specified rights and obligations. The rules for the acquisition, use, and transfer of rights to land, we call “land tenure”; the rules specifying human co-operation in production processes, we call “division of labor” or “work organization”; if tools and technical knowledge are important in any economy there will be rules for their acquisition, use, and transfer.

Two general points emerge: when the rules specifying rights of acquisition or usage of any of these components of an economy are expressions of kinship or political relationships, the economic component is inextricably related

TABLE 3

## ANALYTICAL CATEGORIES AND RELEVANT QUESTIONS IN ECONOMIC ANTHROPOLOGY

## I. TRADITIONAL ECONOMIES

A. *Types*

1. Primitive, without centralized polity (Tiv).
2. Primitive, with centralized polity: chiefdoms, kingdoms, empires (Nupe, Bantu, Inca).
3. Peasant (Malay fishermen, Latin American peasantries).

B. *Analytical Distinctions*

## 1. Organization

- a. Size of economy; technology; natural resource endowment.
- b. Transactional modes (reciprocity, redistribution, market-exchange; dominant-integrative modes distinguished from petty modes).
- c. Production processes: (1) allocation of resources (land acquisition, use, and transfer; labor acquisition and use; the acquisition, use and transfer of tools and equipment); (2) work organization; (3) disposition of produce; (4) specialist services and their remuneration.
- d. Organization and role(s) of external trade (reciprocal gift trade; politically administered trade; market trade).
- e. Organization and role(s) of internal markets and market places (marketless economies, petty market places, small-scale market-integrated economies; resource markets and produce markets).
- f. Organization of money and money uses (general-purpose and special-purpose monies; commercial and non-commercial uses of money; relation of money uses to transactional modes).
- g. Operational devices: record-keeping, accounting, and measurement devices (quipu strings, pebble counts); devices of culture contact (silent trade, border markets, ports of trade).
- h. Prestige economy contrasted with subsistence economy (transactional spheres and conversions; bridewealth; ceremonial transfers; valuables and treasures as special-purpose monies).
- i. The relation of economic to social organization (the place of economy in society): social control of resource allocation, work organization, and produce disposition; social guarantee of livelihood through resource allocation and the provision of emergency subsistence.

## 2. Performance

- a. Number of goods and specialist services produced or acquired.
- b. Level of output; fluctuations in output; frequency of dearth or famine (emergency devices in dearth or famine: use of trade partners for emergency gifts; use of less-preferred foods; emergency conversions, e.g., sale of treasures and people for food).
- c. Distribution of real income: equal or unequal? Why?
- d. Distribution of subsistence goods contrasted with distribution of prestige goods (spheres of exchange; conversion between spheres).

C. *Special Problems Relating to Peasant Economies*

1. The nature of market organization and dependence contrasted with national, developed market economies; why "penny capitalism" is an appropriate description of peasant economy.
2. Peasant economy and culture before and after the Industrial Revolution.
3. The mixture of traditional and market economy; of traditional and modern technology; of traditional social organization and culture and elements of modern culture.
4. Peasant economy and society in contrast to primitive economy and society, and in contrast to industrial capitalist economy and society.

## II. SOCIO-ECONOMIC CHANGE, GROWTH, AND DEVELOPMENT: SEQUENTIAL PROCESS ANALYSIS

A. *Contexts of change and development: colonialism-culture contact, independence-explicit national and village level modernization.*B. *Types of change*

1. Degenerative: cultural disruption and absence of substitute forms of organization.
2. Cash income growth without development: primitive economies becoming peasant; adoption of cash-earning activities with little or no disruption of ordinary life and without concomitant technological and other innovations which diversifies and sustains income growth.
3. Development: sustained income growth for the local community through integration-economic, political, cultural—into the larger socio-economic unit of which it is a part, without loss of ethnic identity or group malaise.

to the social, and we have a *socio-economic* practice, institution, or process. Aboriginal land tenure in parts of Africa are obvious examples, where land is acquired through kinship right or tribal affiliation (Bohannan 1954; Schapera and Goodwin 1937: 157). Secondly, what we call economic organization is the set of rules in force through which natural resources, human co-operation, and technology are brought together to provide material items and specialist services in sustained and repetitive fashion.

(3) A third similarity is the incorporation of superficially similar devices and practices in economies differently organized. Economies as different as the U.S., the U.S.S.R., and the Tiv make use of market places, foreign trade, monetary objects, and devices for measuring and record-keeping.

In summary, all societies of record—those studied by anthropologists, historians, and economists—have structured arrangements to provide the material means of individual and community life. It is these structured rules that we call an economic system. Economic anthropology delineates these social rules of economy by describing activities and folkviews, and analyzing transactional processes and relationships in the small-scale, pre-industrial communities of the underdeveloped world, and makes comparisons between primitive, peasant, and industrialized developed economies. So too with comparing the components and sectors of economy: the allocation of land and labor, the organization of work, the disposition of produce, and the organization and usage of forms of money, markets, and external trade. There are very important differences among economies, however, differences in structure and in performance, and much valuable analysis lies in contrasting them.

#### TRADITIONAL, PRIMITIVE ECONOMIES: STRUCTURE AND PERFORMANCE<sup>8</sup>

The questions about primitive economies of most interest to anthropologists relate to their organization (structure), and to comparisons of their organization with that of other types of economy (peasant, industrial capitalist). With regard to their performance, one can indicate the relatively narrow range of goods and specialist services produced or acquired. The level of output and fluctuations in output can be measured in terms of quantities produced (Deane 1953; Reynders 1963). Input measures can be devised (Salisbury 1962), indicating amounts of equipment used in production processes and work-days employed, and so arrive at some estimates of productivity. Dietary standards can be scrutinized (Richards 1939). Some impressions of the equality or inequality in real-income distribution can be conveyed. Given the absence of Western money and pricing and the relatively few resources used and goods produced, these measures of performance can only be rough indicators stated in terms of the resource and product units themselves.

<sup>8</sup> The literature of primitive (subsistence) economies—traditional economies most different from our own—is richest for Africa and Oceania, for small-scale communities rather than kingdoms and empires, and for agriculturalists rather than hunters, gatherers, herders, etc. Malinowski's work (1921; 1922; 1926; 1935; also, Uberoi 1962) is the single best source. On the economies of kingdoms and other politically centralized societies, see Nadel (1942), Maquet (1961), Arnold (1957), and Polanyi (1966).

It is this smallness of scale, so hard for a modern European to grasp imaginatively, which is the fundamental characteristic of primitive life . . . (Wilson 1941: 10).

There are some useful distinctions to be made among traditional economies. Much of the literature of primitive economies describes those without centralized polities—"tribes without rulers"—Malinowski's Trobriands being the most minutely described case in the literature. In saying that most primitive economies without centralized polity are small, one means several things: that the economy of the Tiv, the Nuer, or the Trobriand Islanders is small relative to modern, nationally-integrated economies of Europe and America; that most (but not all) resource, goods, and service transactions take place within a small geographical area and within a community of persons numbered in the hundreds or thousands. It is true that external trade is common and, as with the Kula, sometimes is carried out over long distances. Typically, however, it is intermittent, petty in amount, or confined to very few goods. It is rare (except in peasant economies) for foreign trade transactions to be frequent, quantitatively important, or essential to livelihood.

There are two other ways in which primitive economies are small-scale. Frequently one or two staple items (yams in the Trobriands, cattle among the Nuer) comprise an unusually large proportion of total produce. It is common for these important staples to be produced within the small framework of village, tribe, or lineage. Lastly, a relatively small number of goods and services is produced or acquired—dozens of items and specialist services rather than hundreds of thousands as in developed, industrial economies.

There are mutually reinforcing connections between the size and other aspects of the structure and performance of an economy. Two widely shared characteristics of the small economies anthropologists study are a simple technology (compared to the industrialized economies of the West), and geographical or cultural isolation (again, compared to those of Europe and North America). The absence of sophisticated machines and applied science, and of the extreme labor specialization characteristic of national economies numbering their participants in the millions, means a relatively low level of productivity. Two direct consequences for primitive economies of their simple technology and small size is that their peoples are sharply constrained in production activities by physical resource endowment (ecology), and that their peoples depend greatly on human co-operation for ordinary production<sup>9</sup> processes as well as emergencies such as famine and personal misfortune. Low-level technology combined with small size and relative isolation results in ingrained mutual dependence among people sharing many relationships: those with whom one is economically involved are the same as those with whom one is involved through neigh-

<sup>9</sup> The extraordinary dependence on immediate physical environment for livelihood made it seem reasonable for an older generation of anthropologists to use classifications such as gathering, hunting, fishing, pastoral, and agricultural "economies". These categories do not classify according to *economic organization*, but rather according to principal source of subsistence, physical environment, and technology. Note that if we used these categories for developed economies, the U.S. and the U.S.S.R. would appear in the same category, both being manufacturing and agricultural "economies".

borhood, religion, kinship and polity. The primitive economy in that sense is "embedded" in other community relationships and is not composed of associations separate from these (Dalton 1962; 1964).

Association is a group specifically organized for the purpose of an interest or group of interests which its members have in common. . . . Community is a circle of people who live together, who belong together, so that they share not this or that particular interest, but a whole set of interests wide enough and comprehensive enough to include their lives (MacIver 1933:9, 10, 12, quoted in Nadel 1942:xi).

Some points may here be underscored: (1) "Primitive" or "subsistence" economies require for the analysis of their *organization* conceptual categories which are socio-economic because material and service transactions are frequently expressions of kinship, religious, or political relationships. (2) Two general features of primitive or subsistence economies are the pervasive social control of production and distribution, and the assurance of subsistence livelihood to persons through the social determination of labor and land allocation and the social right to receive emergency material aid in time of need.

These points have frequently been made before: to Tönnies, primitive economies are *Gemeinschaft* rather than *Gesellschaft*; to Maine, they are characterized by status rather than contract; to Weber and MacIver, they are communities rather than associations; to Karl Polanyi (1944: Chap. 4; 1957), the economy is "embedded" in the society; to Raymond Firth (1951:142), the formula is "From each according to his status obligations in the social system, to each according to his rights in that system."

Primitive economies are so organized that the allocation of labor and land, the organization of work within production processes (farming, herding, construction of buildings and equipment), and the disposition of produced goods and specialist services are expressions of underlying kinship obligation, tribal affiliation, and religious and moral duty. Unlike the economist who can analyze important features of industrial capitalism (such as price and income determination) without considering social relationships, the economic anthropologist concerned with the *organization* of primitive economies finds there is no separate economic system that can be analyzed independently of social organization.

The ways in which tools and implements are acquired, used, and disposed of is another point of contrast between primitive, peasant, and industrial capitalist economies. Typically in primitive economies tools are either made by the user himself, acquired for a fee from a specialist craftsman, or, as is sometimes the case with dwellings, storehouses, and canoes, acquired from a construction group specifically organized for the task. The construction group providing ordinary labor as well as the services of craftsmen specialists is remunerated either by food provided by the host (Thurnwald's *Bittarbeit* and barn-raising in the American West), or with food and luxury tidbits (tobacco, betel), or with these as well as payments in valuables or special-purpose money to the craftsmen-specialists (Dalton 1965a). Western cash is not paid. The making of tools, canoes, and dwellings is an occasional event rather than a continuous activity, and the construction workers do not derive the bulk of their livelihood from providing such services. The tools, canoes, and buildings

when put to use do not yield their owners a cash income. Typically, the implements are used until they are physically worn out, when they are either repaired or discarded. Unlike some peasant economies (Firth 1946), primitive economies have no second-hand markets for tools and buildings.

Polanyi's analytical distinctions between reciprocity, redistribution, and (market) exchange and their application to specific cases have been written up in detail (Polanyi 1944: Chap. 4; 1947, 1957, 1966; Dalton 1961; 1962, 1965c). Unfortunately, they have been misconstrued as applying only to transactions of produce (Smelser 1958; Burling 1962; Nash 1966). These socio-economic categories apply to inanimate resource and labor allocation and to work organization as well as to produce disposition—to production as well as to distribution of goods and craft services (LeClair 1962). It is misleading to regard "systems of exchange" as something apart from production processes because exchange transactions enter into *each* of the three component processes of production (Dalton 1962: 1964).

Consider any production process: automobile manufacturing in the U.S., yam growing in the Trobriands, collective farming in the U.S.S.R., Malay peasant fishing, or cattle raising among the Nuer. All these production lines require the allocation of land, labor, and other resource ingredients to the production process; the organization of work tasks within the production process; and the disposition of the items produced. Among the Tiv, acquiring farm land (in accordance with one's lineage affiliation) is as much a "reciprocal" transaction as yam-giving (in accordance with one's *urigubu* obligation) is in the Trobriands.

#### PRIMITIVE STATES: INTERNAL REDISTRIBUTION AND EXTERNAL ADMINISTERED TRADE

As in other branches of anthropology, the typical unit of analytical interest in economic anthropology is a relatively small group, the tribe, the lineage segment, the village community. There is a small, internal economy to be analyzed whether our focus of interest is a primitive economy without centralized polity (such as the Tiv), a primitive economy within a centralized polity, such as the local farming communities in Nupe (Nadal 1942), or a peasant economy, such as the Malay fishermen (Firth 1946). To be sure, persons or groups within each of these small economies may carry out transactions with outsiders—external trade, tax and tribute payments to outside political authorities—but it is meaningful to distinguish between internal (local community) transactions and those external to the local group, however defined.

Primitive economies which are part of centralized political authority—what Polanyi called archaic societies and Evans-Pritchard and Fortes (1940) called primitive states—have socio-economic transactions in addition to those found within the local community and between local communities (see Figure 1, p. 74). These are of two principal sorts, transactions between the political center and its local constituencies, and external trade transactions between the political center and foreigners (Arnold 1957a, 1957b; Polanyi 1963, 1966). The local constituents pay tribute to

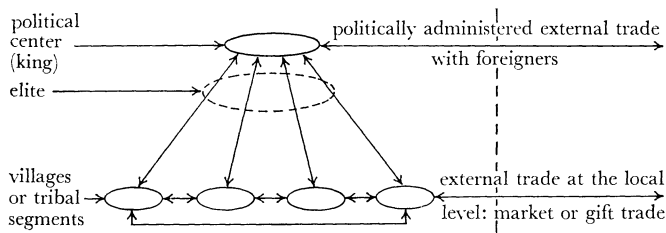


FIG. 1. Socio-economic transactions in the primitive economy within a centralized political authority system.

the political center—ordinary subsistence goods, luxuries reserved for elite usage, labor for construction projects and military service—and usually receive from the center military protection, juridical services, and emergency subsistence in time of local famine or disaster.

Where there is a centralized political authority, there is a redistributive sector which has no counterpart in primitive economies without a centralized polity (i.e., that are not chiefdoms, kingdoms, or empires). Indeed, where there is an intermediary elite between the king (his royal household economy and his domain), and the villages or tribal segments which express their political subordination through tax and tribute payments and other upward transactions, there are socio-economic sectors that some writers call feudal (Nadel 1942; Maquet 1961), although others question the usefulness of so labeling them (Goody 1963; Beattie 1964).

#### PEASANT ECONOMY

Writers on peasantry (Redfield 1956; Wolf 1966) emphasize the special nature of peasant personality and culture as that which distinguishes peasant from primitive: the semi-isolation from urban culture with which it shares religion and (in Europe) language; that peasants and peasant communities are the rank and file, so to speak, of larger political groupings, so that in Latin America, Europe, and India there are political authorities externally located who exercise some formal political jurisdiction over the peasant villages.

It is important to note that if we confine ourselves to cultural aspects such as religion, language, and political subordination, we can point up what is common to an enormous number of peasantries, and, at the same time justify the use of the special category, peasant culture, by showing it is different in these ways from primitive culture. Trobriand Island culture has none of the characteristics so far enumerated for peasants.

To go further, however, requires some special distinctions because of the long periods of historical time over which groups called peasant by social analysts have existed intact, and because there are other criteria used to differentiate peasant from primitive and modern.

One line of demarcation is the Industrial Revolution. Before the Industrial Revolution occurred in their regions, all peasantries used primitive technology, differing in no important way from the technologies used by those groups (Tiv, Lele, Nuer) anthropologists identify as being primitive. Let us call peasant communities as they existed before the Industrial Revolution in their regions, "traditional" peasantries. Then we can point out immediately that traditional peasantries, although differing from primitive

societies in those cultural ways specified earlier, were like primitive communities in their use of simple (machineless) technology, their small units of production (principally but not exclusively agricultural) and the relatively few items produced within a peasant community. In traditional peasantries as in primitive communities, there is the same reliance upon one or two staple foodstuffs which comprise a large proportion of total output, and the same unusually large reliance upon natural resource endowment because of the simple technology used and the absence of complicated fabrication processes. With regard to the size of production units, technology, dependence on physical resource endowment, and the narrow range of items produced, traditional peasant communities resemble the primitive much more closely than they do with regard to culture. Moreover, material performance is roughly the same as in primitive communities, and for the same reasons. The ethnographic record does not indicate that traditional peasantries were typically less poor materially than primitive societies.<sup>10</sup>

What anthropologists mean by peasant culture is clear; what they mean by peasant economy is sometimes not clear.

By a peasant economy one means a system of small-scale producers, with a simple technology and equipment often relying primarily for their subsistence on what they themselves produce. The primary means of livelihood of the peasant is cultivation of the soil (Firth 1951:87).

But this is a perfect description of the Lele (Douglas 1965), the Tiv (Bohannon 1968), and the Trobriand Islanders in Malinowski's time—all have primitive economies. If we are to make analytical sense of the large literature of economic anthropology we need some finer distinctions.

It is as useful to distinguish between peasant and primitive economy as it is to distinguish between peasant and primitive culture. The *economic* organization of a peasant community has two sets of distinguishing characteristics: (1) most people depend for the bulk of their livelihood on production for market sale or selling in markets; purchase and sale transactions with cash are frequent and quantitatively important; and, frequently, resource markets are present: significant quantities of labor, land, tools, and equipment are available for purchase, rent, or hire at money price. It is the relative importance of markets for resources and products and of cash transactions that is the principal difference between peasant and primitive *economies*. It is this feature which gives peasant economies their crude resemblance to the least productive of our own farming sectors and which justifies Tax's appropriate phrase, "peenny capitalism." But in all other ways relating

<sup>10</sup> For many traditional peasant economies (village communities), it is undoubtedly true that real income is no higher than in most primitive economies. But, aside from difficulties of measuring real output, there are complicating features of peasant society which make it difficult to say whether many peasantries had consistently higher levels of output than is typical in primitive communities. Peasant communities, for example, seem invariably to be subordinate units of larger political (and religious) groupings, which means that significant portions of peasant produce and labor are paid "upward" as taxes, tributes, rents, and tithes. The elite recipients of such taxes and tributes channelled portions of them into the creation of churches, palaces, pyramids, armies, etc., some of the services of which were received back by the local peasant communities. Again, the slow growth of improvement in agricultural and marketing techniques in some European peasant communities for several hundred years before the Industrial Revolution may have given some European peasant communities of, say, the 18th century higher incomes than is typical of other peasant and most primitive economies.

to productive activities, peasant economies—especially traditional peasantries—more closely resemble the primitive than they do the modern: small-scale, simple technology, a narrow range of output, a few staples comprising the bulk of output, unusual reliance on physical resource endowment because of the absence of applied science and the technology of extensive fabrication; low levels of output—poverty and material insecurity.

(2) What strikes the economist is that although the rudiments of capitalist (i.e., market) economy are present and important in peasant communities, they are *incomplete* and *underdeveloped* compared to market organization in a modern national economy. By incomplete is meant that within a given peasant community, some markets may be absent or petty—land may be frequently purchased or rented but labor is not, (Chayanov 1966) or vice versa; and that subsistence production may still be quantitatively important in some households. By underdeveloped is meant the absence of facilitative institutions and social capital of advanced capitalist countries: on the one hand, banks, insurance companies, and stock markets; on the other, electricity, paved roads, and educational facilities beyond the elementary school. In peasant communities the extent of economic, cultural, and technological integration with the province and nation is markedly less than is the case with hinterland communities in developed nations.

In summary, peasant society, like primitive society (and also feudalism, *jajmani* in village India, and slavery) is a socio-economic category (Firth 1964a:17). If we include peasantries of all times and places within our analysis, then it is fair to say that peasant culture is more homogeneous and distinctive than is peasant economy (Fallers 1961). The spectrum of peasantries is wide, and contains varying mixtures of primitive and modern institutions. At one end are those in medieval Europe—the Russian mir, the feudal village (Bennett 1962) and some of present-day Latin America, which are peasant cultures (in religion language, political subordination) with primitive economies (because of the absence of market dependence and cash transactions). There are also cases of peasant economy with a primitive culture, as in the early transition period of African groups enlarging their cash-earning production while retaining their tribal organization and culture (Fallers 1961; Gulliver 1965; Dalton 1964).<sup>11</sup>

<sup>11</sup> In this paper I can only call attention to how little work has been done on the economic aspects of peasantry (in the anthropological literature) and suggest that similar cultural features accompany dissimilar economic arrangements in the broad spectrum of peasant societies. There is a great deal more to be said about peasant economy. I am preparing an essay which classifies peasant societies into three sorts. Type I consists of peasant communities which have dependent (non-market) land tenure (such as those under European Feudalism) in which land is acquired by clients from patrons as part of a long-run social and political relationship. Clients reciprocate with obligatory payments of material goods or labor services (farm labor, military labor, road repair, etc.), as well as with more diffuse social and political “payments”—loyalty, respect, homage, ceremonial services. Type II consists of peasant communities of a post-French Revolution sort, in which land tenure is strictly a matter of market purchase (or rental at money price) with no social or political obligations attached to land acquisition or usage. Types I and II refer to communities of long settlement. Type III is a hybrid sort, referring to communities of persons resettled within relatively recent historical times, frequently, as the aftermath of slavery in the Caribbean and the Spanish conquest of Latin America. Kroeber and Redfield, understandably, seized upon cultural attributes to differentiate “peasant” from “primitive” cultures. What remains to be done is socio-economic analysis of peasant groups from an anthropological perspective which takes account of the rich historical literature of European peasantries (e.g., Chayanov 1966) as well as the more recent ethnographies.

### III. COMMUNITY CHANGE AND DEVELOPMENT

The most promising area for fruitful interchange and collaboration between economics and anthropology is the field of economic development. Most development economists, however, are interested in processes and problems of *national* economic growth and development that have little in common with anthropologists' interests in local community social and economic change. But a growing number of economists are working on matters requiring anthropological insight: creating an industrial labor force, transforming subsistence agriculture (Yudelman 1964), devising policies for investment in educational facilities. Others are devising techniques of measurement and analysis to show the connections between socio-political organization and economic development (Adelman and Morris 1965, 1967). And yet other economists are making use of anthropology, sociology, and psychology to analyze—what is for economists—an unusual range of processes and problems entailed in economic growth and development (Hagen 1962; Myrdal 1957).

Matters relating to what I shall call socio-economic change, growth and development at the local community level conventionally appear in anthropology under the headings of evolution, diffusion of innovations, social change, culture change, culture contact, acculturation, and applied anthropology. There are two points about this literature of socio-economic change that I should like to emphasize.

The subject is extraordinarily diverse and complicated. It includes a wide range of complex processes: urbanization, industrialization, commercialization, national integration (cf. Southall 1961, UNESCO 1963). Moreover, these processes take place over much longer periods of time than anthropologists customarily remain in the field, and their analysis requires consideration of the policies of central government which impinge on the small group (village or tribal segment) that traditionally has been the focus of interest in anthropology.

The case studies of socio-economic change reach back to the early days of European colonization of Africa (Schapera 1934; Hunter 1961), Latin America (Chevalier 1963), and Asia (Boeke 1942), when neither political independence was a fact nor economic development of indigenous peoples an explicit intention. The recent case studies are of socio-economic change taking place in villages which are now parts of independent nation-states whose central governments are initiating nationwide development and modernization. The literature includes cases of piecemeal change, where a new cash crop or a new school or a new religion is introduced in an otherwise traditional community (Dalton 1964), and cases of comprehensive community development, such as the famous case of Vicos (Holmberg *et al.* 1965).

Given the complexity of the processes, the large number and diversity of case studies on record, and the changed political and economic national conditions under which local community development now proceeds, it is not surprising that relatively few theoretical insights and conceptual categories with which to analyze socio-economic change have been contrived. Some notable contributions

are Myrdal (1957), Hagen (1962), Smelser (1963), and Adelman and Morris (1967).

Socio-economic change as an anthropological subject is unusual in another way, as well. Many of us who work on problems of development and modernization hope not only to come to understand these processes, but also to use such knowledge to reduce the social costs of economic improvement. Therefore, this extension of the traditional concerns of economic anthropology into processes of socio-economic change and development has policy implications to an extent that is unusual in anthropology (Erasmus 1961; Goodenough 1963; Arensberg and Niehoff 1964).

What is also true is that each of us—the anthropologist, the economist, the sociologist—comes to a novel situation such as change and development in a African village community with two kinds of professional knowledge, the theory of one's subject, and an intimate knowledge of some portion(s) of the real world. The economist (typically) comes with price, income, growth, and development theory, plus his knowledge of the structure and performance of his own and perhaps several other economies. If he is a specialist in economic history or Soviet economy (Gershenkron 1963), he brings with him knowledge of the sequential processes through which England, Japan, Russia, or the U.S. developed. When he comes to examine local development in an African community, he is struck by similarities to and differences from what he is already familiar with.

First, with the exception of agricultural economics, there is no counterpart in conventional economic analysis to the study of local community change and development. European and American villages and townships—the local community counterparts of the Tiv lineage segment or an Indian village—are never the focus of analytical concern. Economics is about national economies and the component activities of business firms and households thoroughly integrated with their national economy through purchase and sale transactions. Immediately we can feed back into our new concerns knowledge that we know is important from our old ones. Empirically, *how do small groups—the tribe, the village—become part of a regional or national economy?*

Similarly, local community change or development seems never to be a “natural” process of immanent expansion of the village or tribe, but rather the local community's response to incursion from outside itself. Whether it is the Conquistadores' invasion of Peru 400 years ago, or Cornell University's invasion of Vicos 15 years ago, or European colonial incursion into Africa, or the slave-raider, missionary, or merchant who comes, the process of community change starts with impingement from without. Therefore, a second question we can ask of the empirical case studies is, *what is the nature of the initial incursion which starts the processes of socio-economic change, and to what extent does the character of the initial incursion shape the sequential changes that follows?*<sup>12</sup>

Most of the ethnographic case studies fall into one of three broad categories that I shall designate (a) degenerative change; (b) cash income growth without develop-

ment; and (c) socio-economic development. The three categories—which are really ideal types—are not stages of progression. Moreover, they are clearly overlapping. Some of the empirical literature fits neatly into these categories, some does not. My point is to make sharp analytical distinctions, and to do so I must oversimplify.

#### DEGENERATIVE CHANGE

Much of the early literature of culture contact consists of European and American incursions which produced decimation, misery, and community degeneration among indigenous groups (Rivers 1922; Japan 1953).

Native [Fiji] society [in the 1880's] was severely disrupted by war, by catastrophic epidemics of European diseases, by the introduction of alcohol, by the devastations of generations of warfare, and by the depredations of labor recruiters (Worsley 1957:19).

By degenerative change I mean severe disruption of the traditional life of a community over several generations with accompanying indicators of novel sorts and frequencies of personal and social malaise. I do not postulate frictionless bliss in the traditional society; but whatever conflicts and malaise were generated by traditional society—warfare, vendetta, sorcery—were coped with by traditional institutions (Malinowski 1959), without prolonged disruption of ordinary life. Where degenerative change occurs, it is, obviously, because the situation is such that traditional institutions designed to deal with traditional sorts of stress and conflict are unable to deal with the novel change because it embodies forces which are at the same time without precedent, irreversible, and overwhelming to traditional organization.

The extreme cases are marked by military conquest and displacement of traditional political authority by conquerors who neither understand nor respect the culture of the society they now control. The indigenous people are unable to resist imposed changes, are prohibited from pursuing rituals or activities which are meaningful and integrative within traditional society, and are made to pursue new activities (e.g., forced labor in mines and plantations) which are not integrative—do not fulfill social obligation and so reinforce social relationships—in traditional society (Steiner 1957).

For the sting of change lies not in change itself but in change which is devoid of social meaning (Frankel 1955:27).

Degenerative situations and the psychological processes of individual and group reaction to them have caught the attention of many writers, perhaps because the consequences are so dramatic. Having lost the primary ties of meaningful culture, social relationships, and activities (Fromm 1941), and having been forced into meaningless activities and degrading helplessness, individuals and groups react to the bewildering changes with fantasy, aggression, withdrawal, and escape (Hagen 1962: Chap. 17; Smelser 1963). And so we have the ethnography of cultural disintegration, from the Pawnee Ghost Dance to Melanesian Cargo Cults<sup>13</sup> and Navaho alcoholism.

<sup>12</sup> A third general point of significance I believe to be the time rate of change which is experienced (Polanyi 1944; Chap. 3). This is not, however, independent of the other features of the transformation process.

<sup>13</sup> Cargo cults are complicated movements expressing several aspects of fission and fusion. Here, I simply want to emphasize that among other things, they are symptoms of malaise that indicate deep misunderstanding of the processes of modernization through which Western goods are acquired.

If one examines these cases of degenerative change from the viewpoint of community development, several features stand out:

(i) *The nature of the initial incursion.* In cases of severe degenerative change, the initial incursion causes cultural decimation: military conquest, political subjugation, and severe disruption of usual activities. A by-product of the incursion may be material worsening, or, indeed, slight material improvement. But these economic consequences are really beside the point because the force of change is perceived and felt to be deprivation of valued activities and the community's subjugation to militarily superior foreigners with hostile intentions and contempt for indigenous ways. The foreigners may come with the intent to deprive the people of gold or land, but typically it is not the deprivation of gold or land which causes the deep disruption.

Not economic exploitation, as often assumed, but the disintegration of the cultural environment of the victim is then the cause of degradation. The economic process may, naturally, supply the vehicle of the destruction, and almost invariably economic inferiority will make the weaker yield, but the immediate cause of his undoing is not for that reason economic; *it lies in the lethal injury to the institutions in which his social existence is embodied.* The result is loss of self-respect and standards, whether the unit is a people or a class, whether the process springs from so-called "culture conflict" or from a change in the position of a class within the confines of a society (Polanyi 1944:157. Italics added).

The nature of the initial incursion seems invariably important, not only to the generation experiencing the initial impact but also—in its shaping the sequences of socio-economic change—to successive generations (Hagen 1962). The group's cultural memory of what they regard as early injustice is long (Schapera 1928), and sometimes is nurtured several generations later (Colson 1949).

(ii) *The absence of new economic, technological, and cultural achievement.* The incursion prevents the society from functioning in customary ways without providing substitute ways which are meaningful to the people in terms of traditional culture (Steiner 1957, Frankel 1955). It is disintegrative to traditional organization without providing new forms of organization which re-integrate the society along new lines (Smelser 1963). These are useful ways to state the problem, but much detailed analysis of socio-economic change needs to be done: What are the sequential processes of disintegration and subsequent re-integration? Which specific features of traditional society are most vulnerable? How long do these processes take? Under what conditions has re-integration taken place? We are here concerned with historical processes to be analyzed in sociological terms. The problems require explicit concern with long stretches of calendar time and with sequential process analysis of old and new economy, technology, polity, social organization, and culture.

Degenerative change does not mean that some people believe themselves to be worse off materially or culturally under the new conditions. Some people are made worse off by any kind of social change. Rather, it means that the old society ceases to function in important ways, most people perceive the changes as worsenings, and in no important area of social or private life is there widespread

absorption of new culture (e.g., literacy), new technology and economy (e.g., new farming methods and enlarged production for sale), of the sorts which create social re-integration. Neither is degenerative change necessarily a permanent state of affairs. Worsley (1957*b*) argues that Melanesian cargo cults, despite their traumatic symptoms of malaise, misunderstanding of European economy, and distorted religiosity, contain the beginnings of wider political organization of an anti-colonial sort which may possibly evolve into more orthodox and productive political activity (see also Hagen 1962).

#### GROWTH WITHOUT DEVELOPMENT

Most of the case studies of community change reported in the literature differ from the one described above in two principal ways. First, the incursion was not severely disruptive of traditional society. The Trobriands (during Malinowski's residence), the Tiv (at the time of Bohannan's fieldwork), and many other groups carried on their traditional activities largely intact for generations after the foreign presence was felt. Second, the peoples became engaged in new cash-earning activities (principally growing cash crops and selling wage-labor), and this was the *only* innovation of importance widely adopted. Subsistence economies became peasant economies as cash earnings and dependence for livelihood on market sale of crops or wage-labor grew, while traditional culture and society remained largely intact (except for those changes induced by the enlarged commercial production or cash-earning).

Here we have the two salient features experienced by a large number of primitive societies: untraumatic incursion which allows ordinary activities, ceremony, and social relationships to continue on much as before; and enlarged cash-earning activities without the concomitant adoption of improved technology, literacy, or any of the other important accoutrements of "modernization" (Gulliver 1965).

I call this situation "cash income growth without development." The community's cash income grows somewhat because of its enlarged sales of crops or labor, but those structural changes in economy, technology, and culture necessary for sustained income growth and the integration over time of the local community with the nation, are not forthcoming. During the period when cash income grows while old culture, values, and folk-views remain initially unchanged (because literacy, new vocational skills, new lines of production, new technology, are not adopted), some characteristic responses are generated:

(1) The use of new cash income for old status prerogatives (bridewealth, potlatch).

(2) New conflict situations (land tenure litigation).

(3) The undermining of traditional arrangements providing material security through social relationships (cash-earning and individualism).

Typically, cash income is earned by individual or household activities rather than lineage or large cooperative group activities (such as canoe building and reciprocal land clearing). Writers on peasant economy (Chayanov 1966; Yang 1945: Chap. 7) stress the economic importance of the *family household* as a production unit for good reasons. The growth of dependence on market sale



of labor or crops for livelihood means the lessened dependence on political heads, extended kin, age-mates, friends, and neighbors—in a word, lessened dependence on local social relationships—to acquire labor or land to use in production processes.

The new form of income, Western cash, is utterly different from anything known in traditional marketless economies. It is indefinitely storable, and so provides material security for its individual owner. It can be used to purchase a variety of goods and discharge a variety of obligations which no money-stuff or treasure item does in primitive economy. Not only a potentially enormous range of European imports—gin, tobacco, canned foods, steel tools, crucifixes, transistor radios—school fees, and colonial taxes, but also traditional subsistence goods (foodstuffs), traditional prestige sphere services, obligations, and positions (e.g., bridewealth), and natural resources (land), and labor, all become purchasable or payable with cash. This is what is meant by Western cash being a “general purpose” money (Dalton 1965*a*). The process of acquisition as well as the transactional use or disposition of Western cash in formerly primitive economies breaks down the traditional separation between spheres of subsistence and prestige goods and services (Firth 1958: Chap. 3; Bohannan 1959).

The use of new cash income for old status prerogatives, new conflict situations, and the undermining of traditional arrangements providing material security are related consequences of earning cash income *within an otherwise traditional setting*. For example, that bridewealth has come to be paid in cash rather than, as formerly, in high prestige items such as cows, indicates the great importance placed on cash (and what it will buy and pay for). The *social* consequences of such displacement are several. Consider the contrasting situations before and after cash has displaced traditional valuables as bridewealth. Indigenously, bridewealth in cows could be got by a young man wanting to marry only by soliciting the required cows from kin, friends, elders, chiefs, i.e., by drawing on social relationships and thus creating obligations to repay them (reciprocate) in some form (e.g., labor service, clientship, etc.). After cash becomes acceptable as bridewealth, young men can raise their own cash and pay their own bridewealth, thus weakening their dependence on traditional superiors.

Indigenously, where bridewealth required the payment of prestige goods, the items (such as cows) could be disposed of by the bridewealth recipients in very few ways. Cows (like kula bracelets) could only be exchanged or paid within the prestige sphere which was narrowly circumscribed. But cash received as bridewealth has no such limitations. It can be used for traditional prestige or subsistence goods, or any of the array of new goods. Bohannan (1959) has pointed out the moral ambivalence which results in the changed situation where bridewealth receipts in cash can be spent on goods in a lower prestige sphere.

#### SOCIO-ECONOMIC DEVELOPMENT

Economists can answer the question, “What constitutes successful development?” with little difficulty. Their unit of analysis is the nation-state, and their base of reference is the already developed nations of North America and Europe. The indicators of successful development from the

viewpoint of economics are impersonal, having little to do with folk-views, attitudes, social relationships, or culture. Development is characterized in terms of the country's yearly percentage rate of growth in gross national product, the size of per capita income and its distribution, and the use of advanced technology in major production lines.

If anthropologists are asked, “What constitutes successful development?” the answer is more difficult. The anthropologists' unit of analysis is the tribal or village community, not the nation-state<sup>14</sup>; anthropologists are not only concerned with economy and technology, but also with folkviews, attitudes, social relationships, and the rest of culture. And they do not use the already developed nations of Europe and North America as a base of reference for successful development. Moreover, anthropologists are analytically concerned with the wider social process of which economic development is a part, and sensitive to social and cultural costs of economic change.

There is no such thing as a small-scale community's development independent of the larger units of economy and society external to the tribe or village. The several kinds of change that constitute modernization all entail integration with external groupings, i.e., increased dependence upon external groups with whom new economic and cultural transactions take place.

Sustained income growth for the local community requires enlarged production for sale to regional, national, or international markets, and a return flow of consumption goods, producer's goods, and social services (health and education) purchased with the ever-increasing cash income. The community becomes economically integrated (and dependent upon) the regional, national, or international economy through a continual enlargement and diversification of purchase and sale transactions. These can be enlarged and made to grow only with the use of improved technology (tools and technical knowledge) acquired or purchased initially from outside the local community. Moreover, the experience of a significant growth in income seems frequently to be a necessary pump-priming condition if traditional groups are to become willing to take the risk of producing new kinds of crops and goods, or old ones with new, expensive, and unfamiliar techniques of production. Primitive and peasant unwillingness to change production is frequently a sensible expression of their poverty and material insecurity. They cannot afford unsuccessful experiments. The old techniques are not very productive, but they keep the people alive. One of the important lessons of the unusual (and unusually quick) development progress in Vicos (Holmberg *et al.* 1965), was that the *Cornell group* assumed the financial risk of planting improved varieties of potatoes. The demonstration effect of the sharp increase in the value product of the new potatoes convinced the people of Vicos to follow suit. A legitimate role for any central government wanting to accelerate local community development is for it to bear some portion of the financial risk of economic and technological innovation.

The local community's integration politically is yet another aspect of community development. But when central government acts only as a tax gatherer, the local community is likely to perceive any governmentally

<sup>14</sup> Clifford Geertz's work is a notable exception.

initiated project to expand community output as a device to increase taxes, and therefore to be resisted. Here too there must be demonstration effects: that government can provide the local community with important economic and social services and confine itself to taxing only a portion of enlarged income forthcoming.

Lastly, there is cultural integration with the larger society: learning new language, new vocational skills, literacy, private and public health practices, and acquiring a participant awareness of alternatives, events, and institutions of the larger world.<sup>15</sup>

What perhaps deserves emphasis is that successful development from the economist's viewpoint is compatible with successful development from the anthropologist's viewpoint. Anthropologists are concerned with minimizing the social costs of community transformation, and with preserving the community's ethnic identity in the new society of income growth, machines, and literacy. But we know from examining the sub-cultures in already developed nations, such as Japan, England, the U.S.S.R., and especially the U.S. (with its unusual ethnic diversity), that the retention of identity in both new and old institutional forms is compatible with modern activities. The point surely is to work with those powerful levers of new achievement which the people themselves perceive as desirable and which induce other positive changes—higher income through new economic and technological performance, and wider alternatives through education. If such developmental achievements are in fact incorporated, those features of traditional culture and social organization incompatible with the new are sloughed off without the personal and community malaise that characterize degenerative change and growth without development.

Social policy has . . . to assure that the individual in losing both the benefits and the burdens of the old society acquire no weightier burdens and at least as many benefits as he had in his previous station (Okigbo 1956).

## CONCLUSION

Karl Polanyi's analytical concepts, insights, and generalizations relate to the socio-economic organization of primitive and archaic economies in which market organization is absent or confined to petty transactions. Here, the components of economy—labor and resource allocation, work organization, product disposition—are expressions of kinship, polity, religion, etc. His analysis is not general in three senses. (1) He did not analyze peasant economies, where market organization, market dependence for livelihood, and the use of Western money are important. (2) He was not concerned with the quantifiable performance of primitive economies, but only with their organization. (3) His analysis of socio-economic change and development was confined principally to Europe (Polanyi 1944: Chaps. 3, 6, 7, 8, 13).

Much of the criticism of his work (and mine) is due to a misunderstanding of the range of economies we are

referring to. Several anthropologists who have done fieldwork in peasant economies (Firth 1946), or in primitive economies at the beginnings of commercialization and use of Western money (Salisbury 1962, Pospisil 1963), look for a universal theory. They complain that Polanyi's categories and generalizations (designed for primitive, static economies), do not fit their peasant and changing economies, and so criticism of Polanyi's work ensues.

If, as with Polly Hill, the agricultural economist who specializes in Ghanaian cocoa farming (1963, 1966), the investigator is interested exclusively in peasant economy, cash crops, and economic growth, and particularly with measurable performance rather than socio-economic organization, then he is being rather short tempered when he criticizes those of us who are interested in economies different from those of Ghanaian cocoa farming, and with aspects of economy and society other than measurable performance.

If, as with Firth and Salisbury, anthropologists are interested in comparative economic performance—how much is produced, how much equipment and labor are used, how income is divided—questions economists put to our own economy, then (in vastly simplified fashion) some of the measurement concepts of conventional economics are usefully applicable, and they fail to understand Polanyi's criticism of conventional economics as inappropriate for analyzing the *structure* of primitive economies.

Some, like Pospisil (1963) and Burling (1962), perceive an economy not as a set of rules of social organization but as economic behavior of individuals and their subjective motivations; when they detect greed and self-aggrandizement they equate these with capitalism and assert that economic "behavior" in primitive societies is the same as in market societies and that Polanyi's conceptual categories are wrong and "romantic" (Cook 1966).

Finally, some anthropologists obliterate all distinctions between descriptive statements, analytical statements, and statements about folkviews, by describing and analyzing the economy, and stating folkviews about it exclusively in market terminology (supply, demand, price, maximizing, capital), and thus quite understandably convince themselves that conventional economics provides all the concepts necessary for economic anthropology (Pospisil 1963).

What must be recognized is that economic anthropology deals with two different sorts of economies, primitive and peasant, under two different sets of conditions, static and dynamic, and with two very different aspects of economy, organization and material performance. Polanyi's theoretical categories are addressed principally, but not exclusively, to the organization of primitive and archaic economies under static conditions. That he did not analyze peasant economies and small-scale economies undergoing change, growth, and development does not vitiate his important contributions to the analysis of non-market economies and the transformations of 18th and 19th-century capitalism.<sup>16</sup>

<sup>15</sup> Gunnar Myrdal's point about the mutually reinforcing nature of developmental activities is indispensable for understanding the processes of sequential change, whether they be degenerative, cash income growth, or the structural changes entailed in successful development (Myrdal 1957: Chaps. 1-3).

<sup>16</sup> Much that Polanyi said in *The Great Transformation* (1944) about the social and cultural consequences of the British Industrial Revolution is relevant to current socio-economic change in underdeveloped areas.

To adduce an analogy that illustrates the point: in the Anglo-America of 1933, the topics handled with Marshallian price and distribution theory (Marshall 1920), and its extensions into the analysis of markets in imperfect competition (Robinson 1932, Chamberlain 1933), were the dominant concern of economic theory (the pricing of resources and products under static conditions in market-integrated national economies). In the Anglo-America of 1968, this remains a concern of economic theory but has declined in relative importance as different questions became important and new theories and conceptual categories were invented to answer them: what determines aggregate national output (Keynes 1936)? What determines the rate of growth of aggregate output over time (Harrod 1952; Domar 1957)? Polanyi's system is akin to Marshall's in its traditional concerns.

Moreover, when Soviet economy began to take its present form, beginning in 1928, special concepts and analyses were invented to deal with what is special to Soviet economic organization and performance. So too with the economics of underdeveloped areas. Economists are used to living in several theoretical universes—price theory, income theory, growth theory, development theory, Soviet economy—which overlap only partially. They do not throw out Marshall because he did not answer Keynes' questions; they do not throw out Keynes because he did not answer Harrod and Domar's questions; and they do not throw out any of these market economy theorists because they did not address themselves to issues of collectivization and central planning in Soviet economy. This

lesson must be learned in economic anthropology if we are ever to progress beyond the stage where deaf men ceaselessly shout at one another. Like the economists, economic anthropologists are dealing with several aspects of several sorts of economies, and need several sets of concepts to understand and measure them properly.

... we have no doubt that the future of economic theory lies not in constructing a single universal theory of economic life but in conceiving a number of theoretical systems that would be adequate to the range of present or past economic orders and would disclose the forms of their co-existence and evolution (Chayanov 1966:28).

## Abstract

While interest in economic anthropology grows rapidly, the creation of a widely accepted theoretical framework is impeded by the persistence—indeed, intensification—of disputes over conceptual issues. Part I of this paper clarifies the issues and explains why controversy persists. Part II attempts to reconcile opposing views by showing how the several different topics that comprise economic anthropology require different sets of analytical and measurement concepts for their fruitful investigation. Part III considers the recent extension of economic anthropology to processes of socio-economic change, growth, and development in communities undergoing "modernization."

## Comments

by CONRAD M. ARENSBERG ☆

*New York, N.Y., U.S.A. 29 vi 68*

I am glad to comment on George Dalton's magisterial "Theoretical Issues in Economic Anthropology." Dalton is the logical successor of Polanyi, and his work represents the culmination of Polanyi's impetus toward the revival of historical institutional economics and the proper theoretical use of ethnographic and historical data in economics and in anthropology. When he writes or speaks he can do so as a man properly accredited and thoroughly professional in both these sciences, the limited one as well as the more general one. His is therefore a most significant voice. CURRENT ANTHROPOLOGY is to be congratulated for offering this statement of the issues uniting and separating the two sciences.

The present statement will go far to end the silly shouting of the deaf in present-day anthropology that Dalton summarizes and rejects. It will put the economic institutions of the human record back into the cross-cultural and analytically comparative perspective that alone can illuminate them. As Dalton rightly points out, no one contends any more that the variant kinship, or social structural, or religious, or political customs among the tribes and civilizations of mankind derive from an elemental

human nature which is enshrined and perfected in European institutions. No more should anyone waste further time over the universality of maximizing, or prestige-seeking, in creating the market system, or temple economics, or kula-ring reciprocities. Dalton's view of these things is squarely based on ethnographic and historical comparison of the social structures, including their interpersonal behaviors and customary rules, of the societies of the anthropological and historical record. Economic institutions are the systematically integrated culture patterns observed to make up the "substantive economics" (younger men today would say the "ecology") of the real societies and the real politics, primitive, peasant, and national, with which they have been reported. Dalton's is an inductive generalization of the cultural record of the kind we all began with, with Polanyi. It is fit to keep company with the empirical classifications and summations anthropology has already effected with kinship systems, political forms, and languages. Dalton has done us in anthropology a service by putting us back on our own path, by informing us once and for all what economics is and is not, and by uniting the two sciences in the service of our understanding of the developing world. He has shown us once again that economic institutions are culture traits, illuminated by the same methods of analysis we have used upon the others and equally subject

to the processes of diffusion, persistence, syncretism, systemic linkage, and evolutionary transformation we have already learned to identify.

by JOHN BLACKING ☆

*Johannesburg, South Africa. 5 vii 68*

This is a very useful and well-written paper, and it successfully carries its point that economic anthropologists need, and are justified in using, several concepts. In particular, the spirited defence of Polanyi is impressive and wholly justified. I have, however, two criticisms of specific points, and a suggestion for an approach which may more effectively bridge the gap between economics and economic anthropology.

Firstly, I am not too happy about the view that patterns of economic organization in "primitive" or "subsistence" economies "are frequently expressions of kinship, religious, or political relationships." Is it not equally, if not more, likely that kinship, religion, and politics are crystallizations and ritualizations of economic processes? I am not advocating economic determinism, but I cannot see any need for kinship *organization* except for purposes of regulating land tenure, ownership of property and usufruct, inheritance and succession, etc. Likewise, religious activity is surely based on physiological experiences to which men attach value and which

appear to belong to another world. Of themselves, these experiences cost nothing (except time for meditation or submission), except in cases where facilities are required for communal worship. There are no good reasons for converting spontaneous worship into the systematic *relationships* of organized religious activity, unless some kinds of experience are thought to be relevant in economic activities; or unless people have come to accept the idea that such experiences can be attained only by, or with the help or instruction of, a group of religious specialists, who therefore have to be paid for their services. Magic and religious ritual are the results of alienation, of personal confidence and awareness shattered and economic organization gone wrong. Moreover, is it too naïve to argue that political relationships are concerned basically with the allocation and control of material resources, even when priestly, military, and other apparently "non-economic" forms of power may be valued?—or have I been in South Africa too long to see politics as anything more than a device for improving or maintaining the economic prospects of groups?

Secondly, I wonder whether Dalton is justified in assuming that local community change comes from without, and that "degenerative change" springs not so much from economic exploitation as from "the disintegration of the cultural environment of the victim" (as Polanyi also argues). Does this not attach too much importance to the role of culture, rather than the quality of personal relationships, in the process of becoming? If personal relationships are not destroyed by the misery of serious economic disorganization, people seem to be able to abandon their culture without loss of self-respect. It is "the culture of poverty," rather than the loss of cultural identity, that hurts. Cargo cults, ghost dances, and the like are religions of the economically oppressed, not of the culturally deprived. "Development" is all too often a euphemism for economic exploitation. Even when the agents of change are kind and well-meaning, their smugness, their self-confidence, or their suppressed arrogance are keenly felt, and poverty and wealth become the yardsticks of cultural validity. The sudden awareness of poverty may make a peasant as dissatisfied with his culture as can be a professor, when he meets a less qualified age-mate who has twice the salary and more time for research because he went into industry. This loss of confidence can lead to a healthy neglect of obsolescent cultural institutions and a desire for more effective reorganization. Malaise and degenerative change set in only when there is no scope for adaptation, generally because it is politically impossible, or because it does not accommodate to the economic plans of the developers. When the interests of community development conflict with

national development, communities invariably suffer. They may then respond in a fiercely chauvinistic fashion, not because they believe that ethnic or cultural identity is more important than individual identity, but because this is the hollow line that larger or national units use to implement their policies, and it looks as if it might bring a greater share of power. For similar reasons, Africans departed from traditional democratic procedures and threw up a series of quasi-charismatic leaders to bargain with colonial governors.

Finally, for the sake of the smoother development of poor nations, if for no other reason, a unitary framework of generalizations about economic activity is desirable. I think this may be possible if we look, after the fashion of Lévi-Strauss, Leach, and others, for the processes which underlie economic activity in many different societies. I do not think it is any more helpful to type societies on the basis of their patterns of economic organization than it is to class them as hunter-gatherers, horticulturalists, or pastoralists. What is needed first are generalizations on the folkviews of each society studied, to discover the structural processes employed in economic activity. Then, in comparison, different societies may be found to employ radically different sets of processes. It is unlikely that the processes used by each society will be entirely unique, though their application in the field of economic life may be. In other words, we may find that the economic systems of *any* societies (industrial, peasant, or otherwise) differ not in kind, nor even in degree, but in the application of a unique combination of processes to overtly economic transactions, one perhaps applying to the organization of economic life processes that others have applied to the organization of religion, kinship, or even music. If we knew what processes in technologically simple societies appear transformed as the economics of industrial capitalism, we would be nearer to solving some of the most pressing problems of modernization in Africa, Asia, and Latin America.

by HELEN CODERE ☆

*Waltham, Mass., U.S.A. 26 VI 68*

This is not only an excellent review article, but it is also being published at an opportune moment in the development of economic anthropology. It is Dalton's contribution to the field to have for all practical purposes created the substantivist-formalist controversy in the sense that it is he who has been the most tireless in keeping Polanyi's views and work before us, it is he who has borne the brunt of the attack, and it is he who has constantly, and again tirelessly, sharpened and improved a number of major substantivist arguments. In this

last connection, the particular contributions of this article are the spelled-out distinction between economic structure and performance and the thorough and systematic discussion of the varieties of the world's economic systems, past and present, and of the challenge to theorizing such variety presents.

It does not seem possible at this point for anyone interested in economic anthropology to avoid consideration and study of both the formalist and substantivist positions and acceptance of one or the other, along with the responsibility this entails to advance the general acceptance of that position according to the rules of our game—namely, that the best hypotheses shall win, that the soundest arguments in logic and empirical validation shall prevail, and that a scientific rhetoric shall be deemed the most effective and efficient.

In this spirit I should like to enter the fray on the substantivist side, in the hope of advancing it to however slight a degree by arguing against what seem to me to be mistaken ideas on Dalton's part about theorizing in general and anthropological theorizing in particular. I agree with Dalton that the attempt on the part of Burling *et al.* to take over and apply the formal (maximization) theory of one section of contemporary economics is, and will continue to be, a failure in the genuinely and broadly comparative field of economic anthropology. I agree not only for many of the reasons Dalton gives but also because a major issue in the field of economics itself is whether its elegant formal theory in microeconomics—the theory in question in economic anthropology—has any empirical relevance whatsoever (see Kaplan 1968).

Dalton is wrong, however, and weakens the substantivist cause, in judging the quest for what he calls "universal" theory as quixotic and in arguing that anthropologists do not take theoretical concepts from science (read "Western" science, if you wish) and that when they do so it is an ethnocentricity comparable to that of discussing exotic religions from the standpoint of some Western Judeo-Christian theology. First, it is precisely because anthropology has universal concepts that it has produced the solid results to date in such work, specifically approved by Dalton, as that on social structure. The concept of role, for example, applies equally well to the Eastern yogi or the Soviet industrial commissar. Other concepts in the field that are less universally useful descent and filiation, for example—are nonetheless "universal" in the sense that they are not restricted to any broad category or type of society. They will apply, where they apply usefully at all, as well to some primitive as to some archaic, peasant, or modern industrial societies, and applying them will confer

precision and comparability on the analysis being made or tested. Second, it may detract from our view of our own originality to say so, but the great majority of the ideas and theoretical concepts we use and test are those of science (again, read "Western" science, if you will, but note that it is the only body of accumulative knowledge and the only source of tried and operational research design and methodology that we have). The borrowings from science are often premature, ill-informed, or mistaken. Applications can be, and have been, clumsy, strained, metaphorical, and just plain wrong. The testing of such borrowed ideas can be, and has been, equally clumsy and inadequate. Such failures or stupidities no more vitiate the over-all process than the entry of the wrong digits vitiates the principles of any mathematical computation. The space considerations of this comment prevent any extended illustrations of points, but it is possible at least to note the career of the borrowed concept of evolution as first applied in an oversimplified way to unilinear sequences, as irrelevantly and mistakenly applied to such areas as that of language, as poorly tested in such schemes as that of Morgan; and as qualified, sharpened, and systematized by association with ecological variables—ecology being another borrowed field with borrowed theory.

The problem, then, of using formal economic theory of the sort described is not that of the irrelevance of any theoretical concept or any body of theory taken from science. Rather, it is either the case that it is poorly understood and used—that is, if it is a scientific theory—or, as not a few of us think, that it is not a scientific theory at all, since it is not about the only possible field of its application, human behavior, and is without empirical basis and untestable.

In contrast to the unscientific character of the formal economics in question, the concepts of Karl Polanyi have universality and empirical reality. Reciprocity and redistribution, for example, can occur in any of the broad types of society previously mentioned and are identifiable and even measurable empirical behavioral phenomena. These and other concepts of Polanyi's scheme give order, broad comparability, and theoretical coherence to our work. That elegance, refinement, and quantitivity are presently lacking does not matter. If concepts, or a loose body of theoretical concepts, are scientific, these formally more satisfying results will one day follow, whereas no amount of formal beauty alone will yield anything but an esthetic rather than a scientific result.

by ERIK COHEN ☆

*Jerusalem, Israel.* 28 vii 68

Dalton quotes Geertz to the effect that "people confuse the generality of a theory

with its ability to be applied universally." He then proceeds to show that several theoretically constructed *general* types are needed for a fruitful analysis of widely divergent economic systems. He concludes that "... economic anthropologists are dealing with several aspects of several sorts of economies, and need several sets of concepts to understand and measure them properly." To my mind, this line of thinking, though acceptable as far as it goes, leaves open a basic theoretical issue, namely, is a *universally* applicable theory of economic anthropology really impossible, unfeasible, or unnecessary simply because the widely divergent *factual* economic arrangements found in different economies necessitate different sets of concepts for their analysis?

Obviously, a universal theory remains necessary as long as we assert, with Dalton, that different types of economies serve essentially the same social function, namely adaptation. Dalton has shown, however, that such a theory would be more complicated than previously thought—indeed, that it will have to be a kind of metatheory encompassing the existing disparate types of primitive, peasant, modern, and other economies.

All the existing purportedly "universal" theories of economic behavior assume rationality on part of the actor. Therefore, as Dalton has shown, the main problem of economic anthropology has been whether such rationalistic models are applicable or useful for the analysis of other than modern industrial societies. He shows convincingly that such models are not applicable over the whole range of economies; from this we learn that a really universal theory should be broad enough to be able to explain both rational and irrational aspects and types of economic systems. Stated differently, it should be a metatheory which would encompass the different theoretical types of economics, constructed by Polanyi, Dalton, and others, as so many points on a continuum or multidimensional property-space of possible types of economic systems. The polar points of the continuum, or the extreme points of the property-space, would represent the outer limits of variability of economic systems—the points beyond which no economic systems capable of fulfilling continuously the function of societal adaptation are conceivable. The continuum or property-space itself would be stated in highly abstract theoretical terms; its main variables would be such variables as rationality vs. irrationality, dependence vs. independence, of the economic system, generality vs. concreteness of economic values, etc. The main theoretical problem would become the clarification of the exact structural conditions in society under which concrete economies would approximate each of the general types of economic systems and thus necessitate the application of one of the various sets of

concepts in the anthropologist's arsenal for their analysis. Such an approach would, in its turn, enable us to refine the crude typologies of societies still so widely used, such as primitive-peasant-modern.

Another possibility implied in such a universal theory would be the analysis of the interrelations between the several disparate spheres of a developing economy. As Dalton has stated, societies under conditions of modernization and development are often characterized by a dual economic system, composed of a more traditional and a more modern, commercialized, economic sphere; each of these is analyzable in terms of a different set of concepts. These spheres, however, are not wholly isolated from each other; there is some conversion between them (Bohannon and Dalton 1965), and the analysis of the mechanisms of conversion might be essential for the understanding of the economic dynamics of developing societies (Barth 1965). By providing a theoretical framework which subsumes systematically all types of economic systems, the metatheory would facilitate the analysis of conversions between types and the prediction of specific kinds of conversions which will occur under different structural conditions. The relationship between the rational and irrational types of economic behavior would thus in itself become a central theoretical issue for economic anthropology.

by PETER VAN EMST ☆

*Groningen, Netherlands.* 19 vi 68

Dalton is to be complimented for his thorough survey of how matters stand at this moment concerning the theoretical approach to economics in non-Western societies. Space doesn't permit taking sides in the theoretical thirty-years' war or trying to reconcile the parties to it; I can only underline a few points. Assuming that man is man, and that we in the West have a culture as much as the inhabitants of Pukapuka, there must be some conceptual tools for analysing and comparing all cultures, at least in a very general sense. Thus, in economics, we can use the concept of "price" everywhere, provided we broaden its meaning to the point that it is nearly—but not entirely!—devoid of meaning. When comparing cultures or parts thereof, economics for instance, we must start with these very general concepts and work down from there to the more specific ones suited to the culture or cultures in question. Of course, at the same time, we must look for the general concepts by working the other way round. Thus "price," or "price-mechanism," has a special meaning as it is used by conventional economists working with market-dominated societies, but this does not mean that it is impossible to find "price" in socialist countries or even in the New Guinea highlands.

Of course, there are differences in economic behaviour and organization between the economies of the modern state, the archaic state, the so-called primitive society, and the various forms of peasant society; but I think that principally these boil down to differences in scale. To say that there exist differences between, say, the economy of the Dutch nation and the economics of the inhabitants of New Guinea is only part of the story. Many traits of the economy of a Dutch household or even a small agricultural hamlet can—or could—better be analysed with the concepts useful for a Papuan household or hamlet than with the conventional concepts of the economists.

Economists, and sociologists, are not so bad as they seem to be. Of course, they still can learn a lot from the anthropologist, but many of them (Marx and Weber, to mention only two) have clearly seen the relations between the economic and other spheres of the society. In other words, we know that our economy is in many respects as “embedded” in society as a whole as primitive or peasant economies. Here again, we must consider the scale. It is not only in India that there is a great and a little tradition, culturally speaking. In fact we can find this dichotomy in a general way in Ashanti and in Europe as well, while in other cases, e.g., the former colonial countries, it takes the form of Boeke’s dualistic economy.

Dalton gives attention to these points, be it in other words. I quite agree with him where he pleads for the study of the relation between the small economy and the great economy; as he puts it: “How do small groups . . . become part of a regional or national economy?” I agree, too, when he says that “Economic anthropology is best done within a framework of comparative economic systems. . . .” This framework, however, must include all systems, great and little, capitalist as well as socialist, archaic state, and peasant; and there must be some basis concepts in terms of which they can be analysed and compared.

by RONALD FRANKENBERG ☆

*Lusaka, Zambia.* 13 vi 68

Dalton’s views on economic anthropology are always both stimulating and clear, and this article is no exception. Above all, one welcomes his determination to keep the discussion within a social framework rather than reduce a social system to a cluster of individual behaviours or dyadic relationships. As I have pointed out elsewhere (1967: 61), Burling, for example, feels no need to classify societies into primitive, peasant, industrial, or slave, feudal, capitalist since fundamentally he does not work within the context of society at all (Burling 1962). I would go further and say that I remain cautious in accepting that

the anthropologist is unlike the economist in finding that “there is no separate economic system that can be analysed independently of social organisation.”

This in fact is where those whom Dalton labels the formalists and Dalton himself come together in the acceptance that conventional economic theory applies even to advanced industrial capitalist society! When Dalton presents his pictorial contrast between the constrained Trobriand yam-gardener and the unconstrained American farmer, he is presenting a difference in quantity rather than quality. Both are constrained, and both are free. It is the nature of the constraints and the freedoms that is the basis of the problem for the student of comparative economic systems. I am reminded of a television discussion on Japanese culture I once saw in Britain, in which that culture was described as being ultra-conformist. “If four Japanese were gathered together like us,” said Dr. Bronowski, “unlike us, they would all wear the same pattern tie.” He had failed to notice that the difference between four Japanese wearing identical ties and four British wearing ties was not between conformity and non-conformity but between two kinds of conformity.

To some extent, therefore, and with characteristic arrogance, I feel that economists need anthropological theories and methods in the understanding of modern capitalist society rather than the converse. This is because economics as a science is based on the assumption or the heuristic device of ignoring the embeddedness of the economic within the social. As Dalton has suggested elsewhere, modern economists have consigned the social concomitants of economic activity to the limbo of Marxism and sociology. It may well be time to bring them back. Lewis (1962: vii) seems to be saying the same thing:

Moreover in studying the market economy, economists have tended to take institutions for granted, and have tended to neglect interactions and conflicts between the market and other social institutions. This has left a gap into which some anthropologists are now sliding. They slip into factories, and study how workers set or react to production norms; they watch directors at work in the board-room; or they study the effects of prestige on occupations. It is clear that the anthropologist’s technique of observation, and his understanding of the inter-relations of social institutions, have an important contribution to make to the study of even the most advanced market economies.

Perhaps, however, a more direct way for anthropology and economics to nourish each other might be through an intellectual dialogue with Marx. This for Dalton is not so much a blind spot as a slight myopia. I miss in the first part of his article an appreciation, critical or otherwise, of Dobb especially *Studies in the Development of*

*Capitalism* [Dobb 1946]), Sweezy (n.d.), or Baran (1957), to name only a few Marxist economists who have concerned themselves with the kinds of problems with which we are here concerned. I have suggested elsewhere (Frankenberg 1967), to Firth’s horrified and heartfelt disclaimer, that Firth himself operates within an evolutionary framework; that Belshaw (1949) and Salisbury (1962) rediscover the labour (time) theory of value; and that Epstein (1962) echoes the *Introduction to the Critique of Political Economy*. They are all Marxists *malgré lui*; it only remains for them to embrace their role and produce (perhaps with Dalton and Polanyi’s aid?) a meaningful synthesis.

There remains, however, a difficulty in the classification of stages and the use of terms like primitive, peasant, and subsistence. Deane, quoted by Dalton, for example, is surely writing about what Dalton would define as peasant societies in Northern Rhodesia, and Allan (1965) has suggested that such economies produce enough for subsistence in bad years (which he estimates to occur only once in six years), leaving them with a surplus for redistribution or exchange in all the other years. Furthermore, it may be an error to abandon the term feudal too readily. I look forward eagerly to Dalton’s promised article mentioned in footnote 11, confident that he will avoid the error of much writing, even by those influenced by Marxism, which tends, if only spasmodically, to confuse the industrial with the proletarian and the rural with the peasant (Fanon 1965, Worsley 1964a), ignoring such phenomena as Arensberg’s peasant townsmen (Arensberg 1937) and Mao Tse Tung’s rural proletariat (Mao Tse Tung 1954). *Cui bono* insists on creeping back into the discussion. The economic concepts of “scarce,” “supply,” “demand,” have to be placed in the context of scarce for whom, supply to whom, demand from whom. In short, the sociological concept of stratification cannot be left out.

Presumably Joy’s application (1967a, b) of linear programming to Darfur (as he would apply it, if given any encouragement, to Tikopia) was published too late for Dalton’s comment. Does the same also apply to the publication in English of Marx’s *Pre-Capitalist Economic Formations* and Hobsbawm’s long and penetrating introduction to it (1964)?

The third part of Dalton’s article, concerned with social change, also invites comment. I think perhaps the point of involvement of anthropology with policy in the new situation is an ethnocentric one; seen from the British standpoint, modern British anthropology has always been heavily involved (Gough 1968). I have recently been classifying papers relating to the Rhodes-Livingstone Institute, and it

and the Makerere Institute were concerned from their inception with practical matters of policy. I am tempted towards the generalisation that the relations between anthropologists and policy-makers have moved from the existential to the essential. Colonial governments tended to scrutinise who anthropologists were and to ask the question as to whether their intentions were honourable or subversive. Once they were satisfied—and within limits—the anthropologist could do what he liked, and if it happened to be relevant to the policy it was supposed to be relevant to, so much the better. Independent governments care less who people are but concern themselves more directly with the essence of what they do; they expect research to be directly useful in policy decisions.

Secondly, I doubt the generality of the statement that community change starts with impingement from without and regard it as perhaps heuristically dangerous. The job of the anthropologist is not to establish a pre-impingement base line but to gain an understanding of indigenous change and then examine how these processes are speeded up, slowed down, or changed in direction by external contact. One doubts that any society is left socially and culturally “much as before” by economic change. There are, however, convincing examples of the intensification of traditional patterns in one of Epstein’s villages, Wangala (Epstein 1962), and in the work of Salisbury (1962).

Dalton makes no mention of studies which to my mind make a major theoretical contribution in this last field—namely Barth’s (1963) on Northern Norway and Worsley’s (1964*b*) on Saskatchewan. Their significance is enhanced because they enable us to compare similar structural processes in very widely different cultural contexts—an aim of *social* anthropology sometimes overlooked by those immersed in a cultural anthropology environment. As Fortes and Evans-Pritchard (1940: 3) said of political systems in a classic statement,

A comparative study of political systems has to be on an abstract plane where social processes are stripped of their cultural idiom and are reduced to functional terms. The structural similarities which disparity of culture conceals are then laid bare and structural dissimilarities are revealed behind a screen of cultural uniformity. There is evidently an intrinsic connexion between a people’s culture and their social organization, but the nature of this connexion is a major problem in sociology and we cannot emphasize too much that these two components of social life must not be confused.

While I would take issue with Dalton’s dualistic conclusion—peasant/primitive, dynamic/static, organisation/material (I remain a trinitarian)—his article is a most useful, clear, and thoughtful contribution. Future students of economic anthropology

can start from here, but they need perhaps three footnotes at least: one French, Godelier (1966*a*), one English, Firth (1967), and one deviant American, Sahlins (1958, 1960, 1962*a, b*, 1963, 1965).

by MORRIS FREILICH ☆

Boston, Mass., U.S.A. 29 VI 68

Dalton’s article is a thoughtful and provocative analysis of matters of interest to all economic anthropologists. In Table 3 he provides an analytical tool kit that most researchers will want to take with them into “the field.” The paper as a whole will probably become required reading in many graduate anthropology courses. With all this (and many additional complimentary statements can be made), the central goal of the paper is not achieved. A reconciliation of Polanyian economic anthropology and that of “the formalists” awaits yet another paper—perhaps by Dalton. Essentially we are presented with *two* approaches to the resolution: (1) that of a combatant, a partisan in the “thirty-years’ war” and (2) that of a dispassionate scholar of comparative economic systems.

Dalton “the warrior” attacks “the enemy” for their quest of a universal theory. He charges them with having an “imperfect understanding of economic theory and its concepts”—providing as proof their use of the economists’ definition of maximization, a definition which cannot be used in non-market economies. He wants them to give up their foolish attempts at formal analysis of subsistence systems because in

traditional, slow-changing, subsistence (and peasant) economies, it is the analysis of socio-economic organization [Polanyian analysis] rather than performance [formal analysis] that yields insights of comparable interest and depth to those got in analyzing religions, polity, kinship, etc. [italics mine].

Anyone who wished to keep this conflict alive might begin by saying that the quest of a universal theory is a noble and proper goal for the game we are playing: *science*. One might go on to argue that the use of concepts from other disciplines does not necessarily entail the use of the same operational definitions for such concepts. We are discussing model-borrowing: the concern is with analogies, *not* homologies; with attempts to sharpen analytic tools to gain predictive ability, *not* simply with attempts to gain insights of comparable depth to those obtained in primitive religion, polity, and kinship. Social and cultural anthropologists wish to swim in deeper depths of understanding. To do so they will grab any concepts that appear fruitful. Further, Dalton might be asked to explain very specifically why, for example, it is not fruitful to think in terms of *surplus* and *supply and demand* in dealing with “primitive societies”? If, in a cows-for-women culture, at time *t*, there were *x*

marriageable women and  $3x$  surplus cows, we might predict that 3 cows will be the modal “price” which is “paid” for a bride. If significant differences were found between this expectation and the actual bride payments it would be tempting to think in terms of “price-fixing mechanisms” which do not allow “the market” to find its level. The argument that such “price-fixing” mechanisms are generally much more complex in primitive than in industrial society, because “tradition rules” in the former and “the market rules” in the latter, is a plausible one, but it is not demonstrated by the ethnographic record. It is only in recent years that anthropologists have really begun to count things—axes, man-hours, cows, etc.—and a continuation of this trend will indicate (I believe) that our mystical, superorganic views of culture (and its puppet man) are not in accordance with the data.

Since anthropologists are borrowing models—that is, using analogies—it is not correct to argue (as Dalton does, p. 66, italics mine):

Describing the potlatch as an investment . . . bridewealth as the price one pays for sexual and domestic services . . . and shell transactions on Rossel Island as cash payments for market purchases . . . suggests they are basically similar to ordinary commercial transactions in our own economy.

Since we wish to understand systems and not simply to gain the understandings achieved by system members, it is not correct to argue (as Dalton does, p. 65, italics mine):

Whether it is analytically revealing to interpret the potlatch, bridewealth, and Rossel Island shells in such fashion depends on the folkviews of these events and usages. . . .

It is possible to go on in this vein, but let us leave “the war” and look at Dalton the comparative economist. Wearing this more objective “mask,” Dalton tells us (p. 66) that a number of important similarities exist in all economic systems:

Whatever the size of the economy it will have several features in common . . . two kinds of goods and specialist services must be provided . . . material requisites of *physical* existence, and goods and services for religion, defense, settlement of dispute, rites of passage, and other aspects of *social* and community life . . . it is useful to regard all communities or societies as having economic systems . . . they all make use of some form(s) of natural resources (land, waterways, minerals), human co-operation (division of labor), and technology (tools, and knowledge of production or acquisition processes). Each of these features is structured . . . to provide the material means of individual and community life.

If important similarities exist among all economic systems, then it would appear useful to ask some similar questions and use some similar concepts to answer them irrespective of the type of system being investigated. Dalton tells us that this is a

good line of reasoning; that (p. 79) if "anthropologists are interested in comparative economic performance . . . some of the measurement concepts of conventional economics are usefully applicable." At this point a real resolution to the conflict could begin. As anthropologists looking to comparative economists for guidance, we would like to know which measurement concepts of conventional economics would be most useful, which least useful, and which completely fruitless. We would like to have some more realistic analysis of economic decision-making procedures in industrial societies. Who is this American farmer who selects cash crops purely on the basis of "physical yield times expected money price compared to money costs of production"? Where does he get complete information to maximize so efficiently? Are his personal likes really so irrelevant? Why is he so different from many an academic who gives up considerable money income to work in surroundings he considers "more agreeable"? If the American farmer really behaved in this way, then income could be adequately defined as net profit. Friedman (1962: 212) tells us, however, that income is actually made up of *money* and *other attractions*. These "other attractions" are considerable in peasant societies, making the peasant often appear "stupid" and "inefficient." Such other attractions may well be considerable in industrial societies, making them perhaps more similar to peasant economies than economists think.

It may well be feasible and indeed fruitful to work toward a unified theoretical framework which is general enough to encompass the *continuum* of subsistence, simple-market, and industrial economies. We look forward to Dalton's aid in this endeavor.

by G. P. GRIGOR'EV ☆

Leningrad, U.S.S.R. 15 VII 68

The theoretical issues proposed by Dalton are acceptable from my point of view. His view that "social organization and culture . . . affect economic organization and performance" (p. 65) is especially fruitful. According to the Marxist theory of economics, this rule is relevant to the study of all types of economy; therefore I do not see the grounds for Dalton's assertion that "the economist . . . can analyze important features of industrial capitalism . . . without considering social relationships" (p. 73).

Dalton's delineation of the main features of primitive economy seems to me accurate. Primitive economy is economy without centralized politics, small economy, with external trade confined to very few goods and the majority of goods produced used within the community. Primitive man is constrained in his production activities by physical-resource endowment and must depend on human co-operation. If we add to these characteristics the ownership by

the primitive producer of the means of production, in contrast to the alienation of the means of production in other types of economy, this picture of primitive economy will fully coincide with Marx's understanding of it. Dalton writes that there is only occasional borrowing of Marxian concepts by anthropologists who write in English. He appears to underestimate the influence of Marx on economic anthropologists.

The study of economic history now begins with the examination of the remains of ancient towns for data on the beginnings of farming and cattle-breeding, metallurgy and pottery. The relations between these innovations, from point of view of economics, are different for different parts of the Old World, and so are their effects on economic and social development. The different effects of the same event (the introduction of pottery, for example) are partly due to differences in ecology, but this factor alone is not a sufficient explanation. Dalton's refinement of theoretical issues and definitions will be very helpful in the future concrete study of primitive economies.

by ZBIGNIEW MARCIN KOWALEWSKI ☆

Łódź, Poland. 15 VI 68

The special value of Dalton's article lies, in my opinion, in his critical and careful analysis of the applicability of categories within political economics—which, as a scientific discipline, is a product of highly developed industrial society—to pre-industrial societies (primitive and peasant) and, within these, to the contemporary underdeveloped societies of the so-called Third World. With reference to these last, however, the extent to which discussion of the applicability of these categories is really fundamental will depend on the degree of penetration of conditions characteristic of a capitalist economy into Third World peasant communities. Several studies by economists and sociologists have placed the majority of peasant communities in Latin America in the area of capitalist economic relations, thus refuting the thesis (e.g., Balandier 1958) that the economies of Third World societies are divided into "modern" (capitalist) and "traditional" (pre-capitalist) sectors. These capitalist economic relations, which penetrate into even the most "isolated" peasant community, are regulated within the community by their own mechanisms, through an internal structure analogous to "metropolis-satellite" economic dependence (Wolf 1955, 1959; Mariategui 1963; Stavenhagen 1963; Frank 1966, 1967); this structure has also been called "internal colonialism" (González Casanova 1964, 1965). In the same way—as Baran (1957) has shown—categories of Marxist political economics seem to be applicable to the economies of

underdeveloped societies, especially, of course, with regard to these economies' undoubtedly unique position in the world capitalist system. At the same time, we must remember that in peasant communities economic relations with the outside world may have a capitalist character even though economic relations within the society are pre-capitalist (e.g., Pozas 1959); in such a case, an analysis of these relations could demand different categories.

Dalton approaches the economic structure of pre-capitalist societies as if it were as autonomous as that of capitalist societies, and as a result his economic anthropology becomes a kind of political economics of primitive or peasant communities. In my opinion, the autonomy of an economic structure which allows it to be subjected to a relatively independent scientific study, whether as a whole or as a regional part of a complex social whole, is the result of a long-lasting historical process; that is why economic structure in primitive societies is not autonomous, as was recognized by Malinowski and Mauss (Lévi-Strauss 1960)—from whence Mauss's theory of the total social fact (Mauss 1954). Godelier (1966*b*) rightly claims that in this kind of social whole, the economist can easily recognize productive forces, but not autonomous productive relations; for here kinship relations function as productive and political relations and thus these three structures are not yet autonomous in regard to each other. Godelier (1965) writes:

The simpler the society, the less possible it becomes to isolate economic factors from other elements of social life, and the greater the extent to which the analysis of a mechanism that appears to be economic is complex, the whole social configuration being found directly in the centre of that mechanism. . . . [Thus] economic phenomena in the womb of a primitive society, simpler than the economy of modern society, are, however, more complicated socially.

Marxian analysis shows that the simplicity of economic categories is the result of a complex process, that simple categories appear on the highest levels of a society's development, in extremely differentiated social wholes, while on the lower levels of development the categories are more concrete, more complex (Marx 1953, Althusser 1965). Mauss (1954) and Malinowski (1922) have shown this in their studies of exchange, and it is also apparent in the property categories I have presented in my study of Andean agrarian communities (Kowalewski 1967).

It is as well that Dalton introduces to economic anthropology problems of the growth of peasant communities. Nevertheless, in building a theory of community development we should not ignore those sociocultural conditions of economic growth which, in vast areas of the



contemporary Third World, create, in the absence of strong social cohesion based on an internal market, its equivalent: the mobilization of peasants into a guerrilla-type revolutionary army, directed by urban intellectuals and regulated by "modernistic" cultural patterns; the organization of rural rear-guard economic support; and the breaking down of the cultural isolation of peasant communities by revolutionary war; all of this being inevitably accompanied by the emergence of the peasantry as a very powerful pressure group (Fanon 1959, 1961; Fals Borda 1965; Torres Restrepo 1966; Lê Châu 1966*b*; Kowalewski 1968) and the acceleration of economic growth of communities by the mechanisms of "ideological mobilization" manipulated by revolutionary authorities (Balandier 1956), as has happened in People's China, revolutionary Cuba, North Korea, as well as North Viet Nam (e.g., Lê Châu 1966*a*). Unfortunately, up till now, anthropology has ignored this, although it does not seem too "deviant" a process.

by EDWARD E. LECLAIR, JR.

*Troy, N.Y., U.S.A. 21 VII 68*

To deal adequately with the points made, the issues raised, the interpretations advanced in the paper under review would require at least as much space as that taken by the original article. This comment can only touch upon a few salient points.

I must begin with an objection. Early in the paper, Dalton suggests, rather gently, that anthropologists writing on these matters, among whom he at that point properly includes me, do not understand economics very well. Later, he makes the point far more bluntly, referring to "their imperfect understanding of economic theory and its concepts." Here, he refers specifically to Goodfellow, Herskovits, Burling, and Cook. It is not clear that Dalton intends to suggest that my understanding of economic theory and its concepts is imperfect. To set the record straight, I earned an A.B., and M.A., and a Ph.D. in economics, concentrating in economic theory, before making the decision to become a professional anthropologist. I am sure that my understanding of economic theory and its concepts is imperfect, but I would think not to a crippling degree.

Moreover, it has always been my impression that the work in economic anthropology of the people mentioned, and of others who are more or less in the "formalist" group, such as Firth, Belshaw, and Salisbury, has demonstrated an increasing understanding of formal theory and has improved in quality as that understanding has improved.

By the same token, I cannot accept Dalton's implied point that if anthropologists only understood economic theory well enough, they would all accept the "sub-

stantivist" viewpoint. At the same time, this does seem an advance of sorts; one could glean from earlier substantivist writing that anthropologists need not bother trying to understand economic theory at all. I'm all in favor of greater understanding along these lines, whatever the consequences.

In one brief passage labeled "The Quest for a Universal Theory," Dalton quotes, from Firth and from me, passages in which we call for what I refer to as a "general theory of economic process and structure." He then suggests that a "single" theory is not possible. It seems to me that "universal theory," "general theory," and "single theory" refer to different things. A general theory dealing with complex phenomena must certainly be complex; perhaps in my original statement I might better have said "general theories," although I would expect that such "general theories" would be interrelated in some ways. Such general theories would be "universal" only in a very limited sense. I argue this because in my original statement on the subject (LeClair 1962:1188) I specified that behind the general theory would lie a series of special cases (or theories), potentially as many as there are different economies.

Twice, Dalton draws an analogy which I consider to be specious. He says that using the concepts of conventional economics "to analyze the non-market sectors of primitive and peasant economies is as distorting as it would be to use the concepts of Christianity to analyze primitive religions." Wallace (1966) has convincingly shown that it is possible to analyze any religion in relation to a single set of concepts. Christianity is a special case of religion in general, just as, as I have argued (1962:1188), Western economy is a special case of economy in general. Wallace's approach to religion might be taken as a model analogue of what I mean by "a general theory of economic process and structure."

A large portion of Part I of Dalton's paper is devoted to re-explication of a cardinal tenet of substantivist economic anthropology: that the economic behavior of "primitive" peoples is so different from that of people in market-industrial systems that the two sets of behavior cannot be dealt with in terms of the same sets of concepts. The facts of differences of behavior between the Iowa farmer and the Trobriander are presumably not much in dispute. It is how those facts are interpreted which lies at the core of the controversy in economic anthropology.

Dalton would have us throw out the theory of choice because "in primitive economies, the constraints on individual choice . . . are extreme, and are dictated not only by social obligation but also by primitive technology and by physical environment." Elsewhere, he tells us that

the "Trobriander is born into a yam-growing economy [and] does not 'choose' to plant yams rather than broccoli." He then tells us that the Trobriander farmer chooses in other dimensions. *All* human choices, however, are subject to constraints of the same sorts mentioned. The Iowa farmer does not choose to plant corn rather than bananas simply because he lives in a corn-growing economy, but his choice is constrained by the environment. He does not choose to plant marijuana rather than corn because of social (legal) constraints. He may choose to continue working rather than retiring partly because of the Protestant ethic or because he wishes to increase his patrimony to his sons. Iowa farmers choose different things for different reasons than is the case with the Trobriander; but within the constraints of various kinds imposed on each, each chooses.

Another related element in the argument is the idea that somehow primitives are to be analyzed differently because their social relationships enter into their economic activity in a different degree than is true of the Iowa farmer. Once again, the only thing that is involved here is that the parameters of choice are different. It is important to study the differences in the parameters of choice; but whatever the parameters of choice may be in a particular situation, presumably the principles of choice remain the same.

There is much more that could and should be said in response to Dalton's paper if space permitted. Nothing in his paper, however, has caused me to alter my basic position as expressed in the paper to which Dalton refers several times (LeClair 1962) nor does it cause me to regret anything I wrote, with Harold K. Schneider, in a book which is now in press (LeClair and Schneider 1968). The reader who is interested in exploring my thinking in greater detail is invited to consult those two works.

by LORENZ G. LÖFFLER ☆

*Heidelberg, Germany. 9 VI 68*

Dalton's contribution undoubtedly constitutes a useful and well-founded review paper. As an attempt to reconcile opposing views, however, it may not be successful. A classification of "economics" and an outline of types is no substitute for a general theory of economics: Dalton himself denounces any claim in this direction as unrealistic. For him there can be nothing but theories relating to special segments of the economic field, and, in consequence, he has to reduce all attempts to frame a suprasedgmental theory to the level of segmental theories or to an even lower level where their value is to be judged against folkviews; the theories of Western economists, on the other hand, are exempt from any such "proof."

It may be useful to recall that sociology suffers from a similar dispute. There are sociologists who cumulate segmental theories but denounce any quest for a theory of society as modern metaphysics. The trouble with anthropologists is that they have folkviews (and ethnography) on the one hand and a yearning for meta-theories (and general anthropology) on the other hand, while segmental theories are at best in the making and generally start either from the folkviews (new ethnography) or the general theories (old ethnology). Whatever the foundations of a general theory—behaviorism, functionalism, structuralism, etc.—its results are not to be measured against folkviews, but only against the possibility of deducing from its set of axioms the fundamentals of the different segmental theories. And in order to yield this possibility, the notions of any suprasedgmental theory must, as a matter of fact, be more comprehensive. It is useless, therefore, to maintain that the proponents of such theories have misunderstood the conceptions used by Western economists: it will be necessary to “misconceive” them in order to proceed. In this sense, e.g., economizing and maximizing may be principles of any economic system, irrespective of whether they are consciously employed (and hence traceable in folkviews) or even calculated (and hence a subject of “higher” economics).

At any rate, Dalton himself has drawn up a list of general features valid for any economic system. Agnosticism cannot induce us to argue a priori that there is no functional or structural connection between these “features,” since at least Marx succeeded in using them as premises for his general theory (on which he built, e.g., his segmental theory of capitalism). This general theory cannot be discarded by the argument that similar theories are *eo ipso* impossible and hence, if drawn, inconsistent; the inconsistency itself has to be shown first. Western economists generally tacitly accept the basic Marxian concepts and avoid the issue by clinging to the segmental level; economic anthropology, however, cannot avoid it. It should and must face it unless we want to perpetuate misconceptions. This criticism may apply (because of the level involved) more to Dalton’s adversaries than to himself. He at least has clearly demarcated the level of Polanyi’s and his arguments, a level which badly needed discussion and has profited from their contributions in a really fundamental way.

by SIMON D. MESSING ☆

Baltimore, Md., U.S.A. 19 vii 68

Perhaps the reason some economists are taking renewed interest in anthropology is a notion that anthropology is a form of witchcraft to be consulted for exorcism when well-laid plans of modernization go

astray. Who else but an anthropologist could explain the accelerated litigation over land tenure? What’s eating the natives who achieve cash-income growth without “development”? What’s gotten into them that they won’t “maximize”?

At the heart of the controversy between the two disciplines appear to be their purposes. The approach of most anthropologists has been to study unabstracted, existential human behavior, with emphasis on economics only to explain ecological survival or to reflect an emphasis on it by the natives themselves. Most economists have sought theoretical abstractions from the outset, and when unaccountable behavior was found it was left to the anthropologists. In recent years the demand of new “developing” nations that both disciplines do something useful to justify their existence has challenged the former division of labor.

Dalton’s review article could pave the way for a more constructive confrontation than the polemics over reified abstractions that have been the practice. One major section, touched only at random in the article, would be the theories of mutual-aid traditions in relation to economic development. Here is a phenomenon of human behavior that appears to be as cross-cultural as may be wished by either discipline.

by P. K. MISRA ☆

Mysore, India. 28 v 68

Economic anthropology has come of age. There is no longer any doubt as to what an anthropologist is to look for when he studies the economy of a simpler society. The controversy has been as regards the theoretical approach and the definition of certain terms. The present paper of Dalton does not improve upon what has already been said on these issues by the advocates of substantive and formal meanings of economics. A dogmatic approach does not help the growth of a science. In Part I, Dalton states why the controversy between substantivists and formalists persists; this is already all too well known. The channels of communication between the advocates of the two points of view which were opened up by the broader definition of economizing by LeClair (1962) and of maximization by Burling (1962) and Cancian (1965) have been ignored by Dalton. Apart from the fact that maximization and economizing are present (whatever may be maximized and economized) in all societies, they are also specifically relevant at least for those primitive and peasant societies where money is becoming important and the economy is becoming involved in a wider network (a phenomenon now being observed the world over). The ideas of maximization presented by Burling and Cancian

give us an opportunity to view the changing primitive and peasant economies within a single frame of reference. They would certainly help to obliterate the paradoxical situation which Dalton sets up by saying that the commercialized sectors of the peasant economies are relevant to conventional economics—as if the two sectors (commercialized and non-commercialized) could be dealt with separately. From the anthropologist’s point of view, economists have studied only one aspect of the market-oriented, price-governed economies, for they have excluded in their analysis matters relating to social organization and other aspects of culture. Economic anthropologists, on the other hand, take all these factors into consideration. With this approach, they can make significant contributions even in analysing the developed modern economies, and hence the scope of economic anthropology need not be limited to primitive and peasant economies.

Dalton’s idea of comparing primitive and peasant economies with industrial capitalism seems highly idealistic. Comparison of institutionalized forms in different cultures is in itself a topic of debate in anthropology, and it is doubtful that a comparison of primitive and peasant economies with industrial capitalism could be achieved as long as they are approached with two different frames of reference.

by MANNING NASH ☆

Chicago, Ill., U.S.A. 21 vi 68

For an essay entitled “Theoretical Issues in Economic Anthropology,” it is surprising how few such issues are mentioned or discussed here. (Compare, for example, the range of issues raised in Firth [1967], Belshaw [1965], or Nash [1966].) Dalton is concerned with a single issue—that of the relevance of contemporary economic concepts and modes of analysis to primitive and peasant economies—and it is one that was probably not even meaningful in the form in which Polanyi first raised it. The question is too global, too unspecified, and too inviting to reasoning by analogy and concluding by polemic.

It is obvious that economic anthropology and contemporary economics are different, and unfinished, intellectual enterprises. Many parts of economics are irrelevant, inapplicable, and unfruitful in understanding primitive and peasant economies. The general structure of thought in parts of neoclassical economics is meaningful, however, outside of the society in which it was developed. The general notions of supply and demand, opportunity cost, axes of choice, have surely enriched the ethnographic description of primitive and peasant economies when intelligently and sensitively used (Salisbury 1962, Epstein

1962, Firth and Yamey 1964). In light of the volume and the quality of work done by anthropologists who selectively use some of the ideas of economics, the global question of the relevance and realism of economics is simply an excuse for avoiding the construction of testable empirical generalizations. The utility of any body of ideas is not resolved in debate but by taking them to the point of encounter with the phenomenological world. Let us get on with the tasks of fashioning the concepts we need to handle and generate our data, rather than exhaust mind and pen in fruitless word-mongering.

In the template analogy between the Trobrianders and modern capitalists used by Polanyi and his followers, there is a great deal of loose reasoning and intellectual slippage. There is a confusion among explaining the institutional structure (obviously only partly an economic question), explaining economic performance (obviously only partly an institutional question), and explaining the cognitive and emotional bases for choice (obviously only partly a psychological question). The comparison of the Trobriand yam-grower, who has no choice but to grow yams (although the amount may be somewhat open), with the American farmer, who can grow broccoli or something else, is a fine example of the confusion. The confusion can be partly resolved by understanding some general principles of social anthropology, rather than the homemade sociology Polanyi uses in his explanations of primitive economies. These principles are simple: (1) choices in all social systems are structurally (technologically and socially) generated; (2) the range of perceived alternatives always and everywhere sets constraints on choice; and (3) within the structure and among the perceived alternatives, actors generally choose to maximize values and minimize penalties (what the values and penalties are is empirical). These principles bring the Trobriander behavior and the American farmer behavior into the same universe and disentangle the confusion of the *magnitude* of choices open with the *act* of choosing among them.

The institutional analysis of Polanyi is rather crude and ad hoc. His principles of exchange are descriptive of some societies, but have little analytical value. As Dalton's Table I indicates, the principles lead to mechanical, schematic, and static understandings at a superficial level. What Polanyi has to offer social and economic anthropology has already been absorbed and superseded, even by some of his adherents (Sahlins 1960). Polanyi was a catalyst in the confrontation of economics and economic anthropology, but like the catalyst in a chemical process he has been consumed in the reaction. "A science that neglects to forget its founders is doomed."

by ROMAN RACZYŃSKI ☆

Prague, Czechoslovakia. 1 VII 68

Economic anthropology as described by Dalton is a real science with a specific methodology (partly borrowed from sociology or from social anthropology) and with a particular object of research (subsistence and peasant economy). The lack of interest in economic anthropology on the part of both anthropologists and economists is due to its interdisciplinary character. Neglect of interdisciplinary studies is a weak link in the contemporary study of human society. A need for integration of the sciences of man has often been expressed. In the case of co-operation between anthropologists and economists, Yamey (1964) has set forth and theoretically justified a complex approach to subsistence and peasant societies. Important contributions to the field of economic anthropology were published in *Readings on Economic Sociology* (Smelser 1965), in the series *Working Papers in Asian Studies* (Bowen 1967), and in the Working Papers of the Economic Development Institute (Hill 1965).

A special problem is the terminology. The term used to refer to this branch of science is sometimes "economic anthropology" (especially in the Anglo-Saxon countries), sometimes "economic sociology" (especially in continental Europe). If the term "anthropology" were to be used for subsistence and peasant society and "sociology" for industrial society, the problem of determining the boundary between these two types of society would still remain to be resolved.

Another terminological difficulty arises in the case of the terms "socialist" and "communist." Socialism is a class society system (with workers, farmers, and working intellectuals) in which everyone works according to his capacities and receives according to his merits. Communism, on the other hand, is a highly developed industrialized society without classes in which everyone works according to his capacities and receives according to his needs; such a society does not exist anywhere. Again, to determine which society is capitalist and which socialist is, in my opinion, even more difficult than to distinguish a subsistence or peasant society from an industrial one. Furthermore, "Soviet socialism," mentioned several times by Dalton, is different from the contemporary Chinese, Czechoslovak, or Yugoslav points of view. Therefore the only antithesis of "subsistence and peasant society" which can be generally recognized is "industrial society."

by JOYCE F. RIEGELHAUPT ☆

Bronxville, N.Y., U.S.A. 30 VI 68

While I could not agree more with Dalton's statement that economic anthropology must "progress beyond the stage where

deaf men ceaselessly shout at one another," I am not sure that this paper does in fact significantly enhance communication. Too much effort continues to be expended arguing with his (and Polanyi's) critics over who is right and who is wrong.

The task of economic anthropology would be greatly eased if the analysts did not try to deal with primitive and peasant societies together. Despite the fact that anthropologists study both types of societies, their economies are of very different sorts and should be handled as distinct conceptual categories. Specifically, this tendency to see the subdiscipline as comprising primitive and peasant economics has led the anthropologist to study peasant economics as though they were confined to the "little community."

By focusing on peasant culture rather than peasant society, Dalton obscures the integration, by definition, of the peasantry. In his comment,

Most development economists, however, are interested in processes and problems of national economic growth and development that have little in common with anthropologist's interest in local community social and economic change.

he reflects a key component in anthropology's inability to deal with peasant economics. Substantial "local social and economic change" for the peasant community is possible only within the process of "national economic growth and development."

The integration of peasantries into larger systems can be clearly noted in Dalton's Type I and Type II peasant economic systems. These different forms of peasant society (but not necessarily culture) developed as the entire society changed: specifically, through the impact of the industrial revolution and the commercialization of agriculture (see Moore 1967, Tawney 1967). With respect to Latin America, Frank (1967) develops further the interrelationship between the economic system and policies of an entire nation and the economics of local life (see especially Chap. IV, "Capitalism and the Myth of Feudalism in Brazilian Agriculture").

Three further comments: (1) While it is useful to understand "traditional primitive" systems in order to understand change, the economic anthropologist must be especially wary in asserting that he has in fact knowledge of the "traditional" (i.e., untouched by modern economic and/or political forces) patterns (see Marshall 1968). In *The Great Transformation*, Polanyi distinguished four modes of allocation: reciprocity, redistribution, market exchange, and *householding*. To Polanyi, "householding" is a form of economic activity found in peasant-type households with the social organizational principle of "autarchy." Polanyi credits Aristotle with first recognizing this system, and he writes (1944: 54),

production for use as against production for gain [is] the essence of the household proper; yet accessory production for the market need not, [Aristotle] argues, destroy the self-sufficiency of the household as long as the cash crop would also otherwise be raised on the farm for sustenance, as cattle or grain; the sale of the surplus need not destroy the basis of householding.

This comes very close to Chayanov's (1966) description of the peasant economy. For a further discussion of the appropriate theories of peasant economy see Dandekar (1962) and Georgescu-Roegen (3) The dynamics and processes of peasant economic life, particularly under the pressures of modernization, should be analyzed with greater attention to both the ecological situation of the peasantry (Yengoyan 1966) and the relationship between the distribution system and the production system. Recent work in Brazil has suggested that the role of peasant production in agricultural development can best be understood in terms of the types of distributive networks available (Forman and Riegelhaupt 1968). There, the distribution system creates certain demands which force changes in the production system and in the position of the peasantry. Specifically, in examining peasant society, where the relationship between subsistence and marketing is critical, the economic anthropologist must devote more attention to the tie between systems of production and systems of exchange; too much attention is focused on the distributive networks alone.

by RICHARD F. SALISBURY ☆

Montreal, Canada. 1 VII 68

My own disagreement with Dalton is virtually complete. I agree that Polanyi's 1957 classification of economies into three types is useful, but only to help beginning students appreciate the different repertoires of superficial economic forms found in different societies. Polanyi was moving away from classifying whole economies in his final work on Dahomey, but Dalton's paper, merely attempts to elaborate the threefold classification. My own views on theory (Salisbury 1966) will be published at length shortly; here there is space for only a brief comment on the false assumptions on which Dalton's article is based.

"In subsistence (non-market) economies, the question of choice among real alternatives does not arise in such explicit fashion" (p. 67) is simply not true. It is an ethnocentric assumption of observers who see only the difference between their own culture and that observed and fail to recognise the diversity within other cultures. It is true that ethnographers have paid little attention to this diversity until recently, but it has always been there. The calculations of subsistence farmers involve a wider choice of alternative crops, detailed planning of sequences of cropping to insure

against variable climatic conditions, planning of labour allocations to smooth out crisis needs, and planning when and how to seek access to the extra-familial labour market taking into account the costs of eventual reciprocation—all of which are more complex than many problems for which economists use linear programming. Studying such planning in detail, and generalising the principles involved, takes one directly to "the most abstract and mathematical of the social sciences." The behaviour studied by anthropologists, and from which theoreticians try to create their decision-making models, is *exactly* the same as that studied by economists. It is exactly those economists who reason most abstractly—I would add Adam Smith and Keynes to Dalton's list—who have the most to teach anthropologists. Anthropologists have most (potentially) to contribute at this level of theory.

Two areas where models based on anthropological observation of micro-economic decision-making could contribute are those of (1) optimising under conditions of uncertainty and (2) optimisation of returns when parameter conditions (technology, political context, etc.) are not fixed. The strength of conventional economics is in making determinate analyses "given cost and demand schedules for a firm"; predicting optimal levels in conditions of uncertainty is a major concern of modern economists. Individual subsistence farmers and peasants make such predictions constantly; anthropologists can study the reasons for their successes and failures and can generalise about them (cf. Foster 1965).

Parameter conditions are taken as given and extra-economic by traditional economists, who tend not to discuss changes in them. Anthropologists by training include the economic parameters in their analyses. Using methods of "controlled comparison" (following Eggan and Nadel) they can demonstrate how parameter differences affect economic choices, sometimes simply, and sometimes by restructuring the entire situation (cf. Goldschmidt *et al.* 1965). My own interest is in comparing a few societies at different times, maintaining close historical control. Such studies lead to models of economic development very different from Dalton's one of substitution of "modern" economies for "traditional" ones. Development is a process of successive single innovations. They are first adopted as advantageous within an existing structure of costs and benefits, uncertainties, and competing interest groups. Then, as information about them spreads, entire patterns of choice are restructured to incorporate them. Only when we understand the complexity of the decision-making which underlies the wide behavioural variation found in any society at any one time can we be in a position to predict the

reactions to parameter changes. Dalton denies this internal variation to "non-market systems"; it is not surprising that his "theory" of development is a descriptive typology of incommensurable "economies."

He is right in deploring the current scarcity of contributions to theory made by economic anthropology. It is unfortunate that his paper does nothing to remedy the situation.

by H. K. SCHNEIDER ☆

Appleton, Wis., U.S.A. 18 VI 68

Dalton's paper is obviously an answer to his critics, produced, it would seem, by the goading of Cook (1966: 323), who accused Dalton of ignoring those who disagreed with him. As such, it piqued my interest, because I hoped it would raise the level of discussion of theory in economic anthropology from that of the "deaf men ceaselessly shout[ing] at each other" that Dalton describes. I am disappointed. As far as I can see, all that has been added is further clarification of Dalton's uncompromising approach, making easier its criticism by formalists. Since neither Dalton, as leader of the substantivists and spokesman for Polanyi, nor the formalists can come to terms, it is to be hoped that this discussion will at least benefit those who are attempting to find a proper theory or theories for economic anthropology.

I would like to direct my remarks to the first two sections of this paper and ignore the third on economic development, which stands or falls with Dalton's theory. First, however, I feel the necessity to comment on the manner in which Dalton presents his case, because I think it has contributed strongly to the polarization of the issues. His language is ungenerous ("Some anthropologists seem not to understand that conventional economics . . . does not deal with what anthropologists mean by human behavior . . ."; "The theoretical portion of economic anthropology has been poorly done"; "The second reason some anthropologists think conventional economics is applicable to all economies is—to speak bluntly—due to their imperfect understanding of economic theory and its concepts." He is addicted to dogmatic pronouncements ("The new form of income, Western cash, is utterly different from anything known in traditional marketless economies"). He misrepresents his critics; e.g., he accuses Burling of describing maximization as purposeful behavior, when Burling's argument is more complicated than that, giving attention to the idea that this is an assumption which is made about behavior, not a psychological postulate (Burling 1962: 813):

The assumption that it is money that is being maximized is only a convenient simplification

in line with the general attention of economists to those instances of choice and behavior in which money is involved.

In short, Dalton argues by means of ex cathedra assertions from a dogmatic position which assumes his critics to be ignorant. Those critics, such as Polly Hill, whom he accuses of being short-tempered, should not be blamed for being tempted to answer in kind.

At the heart of Dalton's position is the claim that there is a difference in kind between capitalist and "primitive" economies, that economic theory has been developed explicitly to explain capitalist economies, and that therefore, economic theory is not applicable to primitive economies. Ultimately this question will be settled by explaining "primitive" economies with so-called capitalist specific theory (as Massell [1963] has tried to do with my Turu data), but for now the discussion must confine itself to the level of logic and fact. Here the first thing one must do is appeal to the optimism of science, which does not declare any theoretical venture as unquestionably illegitimate. The theory's failure to predict will disprove it better than Dalton's insistence that it is inapplicable. As Frankenberg says well (1967: 65):

He [Dalton] seems to me to show the limitations of formal economics rather than damage the anthropologists' use of it, for . . . the anthropologist has been eclectic and creative. And wisely so.

The issue here is a very old one: the choice between an inductive or deductive approach. Dalton has a predilection for the inductive and therefore, also, it seems, for the particular over the general. Hence, he sees the capitalist economy as a special type of system and he also is most aware of the way economic theory is specialized to fit it. He fails to see what is general in this particular theory. In fact, he ridicules the search for general laws. I don't know how such an extreme relativistic stand can be challenged. Bohannan, who has taken a similar stand with respect to the comparability of cultures, is a logical collaborator with Dalton (Bohannan and Dalton 1965) and is, likewise, irrefutable. I doubt that even predictions in different societies based on general theory would alter this point of view. Even allowing for this, however, it is anachronistic to see Dalton categorizing the world as consisting of Western (capitalist economies) and "primitives" (all the rest).

Probably the greatest contribution of this article is to bring to the fore the question of the relation of society and economy. Dalton makes much of this, and rightly. He notes that economists ordinarily ignore society but that anthropologists cannot. He argues that since societies differ, economies are differently adjusted to them, and ultimately capitalist economics will

not apply. To Dalton, the economy in primitive societies is "embedded," by which he means it is made to serve social ends. In this article, for the first time as far as I know, Dalton invokes the conventional wisdom of the *Gemeinschaft-Gesellschaft* ideal-typical typology of Tönnies to bolster his argument, pointing out that it has been paralleled by Maine's status and contract and Weber's communal-associational distinction. To these I would add Riesman's tradition-directed and inner-directed and Durkheim's mechanical versus organic solidarity. Each of these polar categories contrasts primitive and civilized societies, most generally in the following way: in the former, relations among people are prescribed, obligatory, altruistic; in the latter, relations are achieved, voluntary, selfish.

I have dwelt upon the Tönnies distinction for the purpose of challenging it and thereby also challenging Dalton's position at a most fundamental level. The primitive-civilized dichotomy is spurious as used by Dalton and derives from his and Polanyi's tendency to think in evolutionary terms. The categories can be preserved while rearranging them along lines suggested by game theory. Society, or any portion of society, such as an institution, can be thought of as a game, that is, as having the essential structure of a game. This structure contains *rules* which govern the play (including the sanctioning elements, whether specialize or diffuse), *goals* or payoffs of the play, and *strategies*, means of achieving the goals within the rules. (Games also seem to require an *ideological* or religious component, belief in the game.) This "game analogy" approach, as I prefer to call it, is parallel to Firth's conceptions (Firth 1964b: 45) of social structure (rules), social organization (strategy), and goals, or the modes of conceptualization of Leach (1960: 123), Hart and Pilling (1960: 14-30), and others. Tönnies' *Gemeinschaft*, or Durkheim's mechanical solidarity, is the rule component of the game, and *Gesellschaft*, or organic solidarity, is the strategic component. What this theory says is that *all* societies have both rules and strategies, all are communalistic (the rule system) and associational (the strategic). What we do when we type societies as either civilized or primitive is focus on strategy in the former and on rules in the latter. This, I think, confuses the issue, which is precisely what Polanyi and Dalton are doing. Incidentally, the theory I have just outlined should be differentiated from that of Belshaw (1965: 4), who makes the point that ". . . all enduring social relations involve transactions, which have an exchange aspect." That is, what we ordinarily think of as "social"—relations of dependence—are of the same order as exchange of goods and services and are rule-governed in the same manner as the exchange of goods and services.

Interestingly, Dalton makes reference to games, but only to point out that each is

different from all others, thus bolstering his point that comparing games is futile. I, in contrast, point out that behind the apparent differences in games is a structure which is comparable, as is so brilliantly demonstrated by Von Neumann and Morgenstern (1964: 46).

Actually, Dalton's views are very close to mine, as is shown in his claim that Polanyi regards the economy as a set of rules of social organization, adding that the proper study of economic anthropology is the difference in these rules. I think there is much to be gained by studying rule variations and how they affect strategies. I see little of scientific benefit in studying the variations in rules for their own sake, but Dalton's failure to see strategies as universal in human behavior means that logically this is what he is urging upon us. If one sees choice only in our society, as is the case with Dalton, one cannot use a game analogy.

Dalton makes much of the necessity in economic anthropology of the study of the relation of economy and society rather than studying choice-making, as economists do in our society; but there seems no reason why both cannot be done. One could study the process of strategic choice in terms of maximization of utility within a certain frame of rules, or one could study how choices are affected by the rule structure. I do not think Dalton would accept this, however, because to him economy is merely the system of distribution of wealth and society is the whole range of dependency relations in a community, all of which, except in capitalist society, are altruistic. When he says we ought to study the relation of society and economy, he means we ought to study (teleologically) how the distribution system serves the society.

Up to this point, I have criticised Dalton's position and suggested how it could be revised to produce a more adequate economic anthropology. I would like to finish by discussing some of the fallacious points he makes to bolster his theory. The first is his claim that choice does not amount to much in non-Western economies. Coupled with this, Dalton seems to make the error of which he accuses Burling, that economic choice is deliberate, purposeful, objective. We can deal with choice theoretically in the same fashion as maximization—as an assumption of the theory; that is, we can assume that people make rational choices without having to believe that people are rational in some absolute sense. Whatever produces a maximizing outcome may be considered to have been a rational choice. Dalton gives the impression that choice is a luxury, or a component only of capitalist societies—that in non-Western economies rules take care of the distribution of wealth and choice is unnecessary. (His additional defense, that wealth is so scarce that there

is not much to exercise choice about, is, I think, simply a misconception, about which more below.) However it is conceived, wealth is scarce, and it must be allocated between alternative ends. The process of allocation can be dealt with as choice, and so choice must exist in non-Western economies (which are therefore not primitive). If we include, as I have done, the exchanges found in personal relations, the range of choice is far greater than is ordinarily supposed of such economies.

One of the defenses which substantivists have often resorted to is the extraordinary claim that primitive economies are "subsistence" economies, in which each household produces the food necessary to feed its members. A society whose members could expect automatically to produce the food necessary to feed themselves each year would be a paradise. My studies of the Turu (1964a: 60) showed that during the good production year of 1959-60, fully 50% of the households in each of two villages did not produce sufficient food to feed their members. Because food supplies were short, all sorts of decisions were forced upon the members: whether to sell livestock to obtain grain, which livestock to sell, whether to invest livestock to increase the land-labor resources of the household to prevent a recurrence of shortages, how to square these capital investments with the aim of maintaining and increasing the number of cattle owned by the household, etc. Dalton, of course, asserts that such economies are too small-scale for the range of choices to be comparable to those in our economy. I think that he is wrong and that we have not seen the complexity or quantity of alternatives because we have not looked for them. What is more, I am sure I am not alone in making this empirical claim. Joy, an economist, verifies it (1967a: 36):

As I myself observed when I worked among the Acholi and peasant Punjabis, the range of choices of farming system, even in the most constraining environment, can be large and complex, involving as it does a nearly infinite permutation of cropping patterns and sequences, fallowing and cultivation practices, and so on.

Turning to another point, Dalton says (p. 78) that

Indigenously, where bridewealth required the payment of prestige goods, the items (such as cows) could be disposed of by the bridewealth recipients in very few ways. Cows (like kula bracelets) could only be exchanged or paid within the prestige sphere which was narrowly circumscribed.

This is highly debatable. Dalton makes no reference to any of the things I (Schneider 1964a, b) or Barth (1964: 69-81) have written on this subject, so that I must presume he has not read them. I presume Dalton would also reject my claim that African cattle are money (actually part of a

monetary system), but I would like to hear on what grounds, since they fulfil all the functions of money as he and others describe them.

I suspect that cattle would be rejected as money because they don't look like money; and this is the essence of Dalton's whole position. Non-Western economies are not analyzable like our economy because they don't look like our economy. This is the ultimate in cultural relativism, or particularism, and is utterly contrary to a scientific approach. It suggests that the solution to this formalist-substantivist controversy is to stop trying to decide on one or the other approach and recognize that one, the substantive, is essentially historical, and the other, the formalist, is essentially theoretical and scientific. They can exist side by side.

by D. P. SINHA ☆

*Mount Hagen, New Guinea.* 16 vi 68

Dalton has done an admirable job in calling our attention to the major controversy in economic anthropology and to the need for adequate theory to explain the economy of primitive and peasant communities. I particularly welcome his attempts to distinguish primitive economy from peasant economy and to offer an analytical framework for economic anthropology. In this brief comment, however, I shall confine my observations to the areas of disagreement rather than those of agreement.

Like Dalton, I believe that Polanyi has made most outstanding contributions to economic anthropology, but unlike him, I do not see the polarity between the approaches of Polanyi *et al.* on the one hand and Firth and others on the other. For one thing, Polanyi dwells on primitive, static economies, while Firth deals with primitive-peasant, both static and changing, economies. The real difficulty is that the anthropologist handling contemporary field data does not run across static societies—not even in the highlands of New Guinea, where I came in search of one nearly a year ago!

Dalton states that economic anthropology is "best done in the framework of comparative economic systems," and I think most of us would endorse this statement. His belief that comparison with historical economies is more relevant than with modern economic system(s) warrants scrutiny, however. Insofar as an anthropologist is concerned with a static economic system, this approach seems productive, but using it to analyse the economic systems of contemporary primitive or peasant economies, most if not all of which are in the process of change, places one in a grave theoretical and methodological dilemma. This dilemma, I believe (Sinha 1967, 1968), can only be resolved by a two-level

approach, describing the traditional system in the light of historical economies and analysing the processes of change with reference to modern market economies.

Dalton asserts that generalizations about change and development have to be based on firm understanding of the traditional socio-economic organization. This is true if one asks questions, as he does, addressed to the receptivity or resistance to innovations or impacts on traditional social structure. If, however, one poses the question—"How have the features of the modern self-regulating market been introduced into the traditional economy?"—one must emphasize the principles of modern economy rather than those of traditional economy.

Much of the literature in economic anthropology indicates that traditional economies operate under the constraints of social, technological, population, and natural resources, and that it is the interrelationship between these factors which produces the broad configuration of their economic systems. Dalton accepts this, and yet in his excellent table summing up the relevant questions in economic anthropology he does not include as an area of inquiry the interrelationship between available and *utilized* social, technological, and natural resources in a traditional economy.

Dalton's observation that the most promising area for fruitful interchange and collaboration between economics and anthropology is the field of economic development deserves more consideration than he gives it here. By examining the context and types of economic change, he convincingly shows that mere contact, intervention, or introduction of innovations is not enough to ensure economic development in a community. He suggests, and I find myself in full agreement, that economic development in traditional communities must be measured in terms of its "sustained economic growth and its integration to the larger socio-economic unit of which it is a part without loss of ethnic identity or group malaise." He leaves implicit, however, the most crucial issue in economic development, that is, planning; for the kind of development he postulates would only be feasible by planned change. Here, I believe, economic anthropology has a great deal to contribute.

by ARNOLD STRICKON ☆

*Madison, Wis., U.S.A.* 29 vi 68

Although I basically agree with Dalton's analysis, I believe that the issues in the economic anthropology debate have become increasingly confused rather than clarified as the argument has continued. I think that the essential issues are simple rather than complex. The apparent complexity of the argument derives from the

fact that we have tried to deal with these issues all at once rather than taking them one at a time. I will treat the following points: (a) economic concepts and their applicability to non-market economies; (b) market theories and their applicability to non-market economies; (c) economic concepts and social theory; (d) theories of primitive economics; (e) assumptions about the nature of individual motivation.

*Economic concepts and their applicability to non-market economies.* We are all agreed that the study of economics has to do with the production and distribution of goods. We also agree that the economy of any society consists of the activities and things allocated to these ends. Obviously, on the ground, these things and activities consist of a multitude of unique events. Any sort of generalization is impossible at this level of abstraction. It is necessary, therefore, to set up categories into which the specific activities and things can be sorted and then dealt with as aggregates. Economic concepts such as labor, production, distribution, financing, investment, supply, profit, etc. represent such categories. Whether different phenomena are to be considered similar or not depends on the degree to which they satisfy the requirements for assignment to a particular category.

Are economic concepts applicable to primitive societies? Some of them are and some of them are not. In all societies, for example, there are things (tools, land, etc.) which we may accurately assign to the category of "capital goods," but in some societies there are no phenomena which will satisfy the criteria of profit, taxes, interest, etc., as these are rigorously conceived.

*Market theories and their applicability to non-market economies.* As Dalton points out, there is no one economic theory, but rather many theories oriented to special needs and problems which shift over time. The question, therefore, is not really the applicability of economic theory, but rather the applicability of a special kind of economic theory, i.e., market theory—which in itself consists of a number of special theories.

The question of the applicability of market theory to primitive economies must be rephrased. The question must relate to the applicability of specific market theories to such systems. These theories are, commonly, not stated in prose (although they may be so translated) but rather as mathematical formulas. These use certain variables (i.e., the operationalized form of the concepts) which can be "filled" with quantitative data. The equations (ideally) describe a relationship among these variables such that changes in one, quantitatively stated, can predict changes in one or more of the others, quantitatively stated.

It is clear that an equation that predicts (e.g.) money price as a function of money costs, money profits, and demand cannot predict anything about activities in an

economy which has neither profits nor money. The question as to whether or not market theories are applicable to primitive economies can be answered negatively on strictly operational grounds. Primitive economies do not present referents for at least some variables; *ipso facto*, the equation (i.e., the theory) does not apply. This would be the case whether or not some economic concepts are applicable to primitive societies and whether or not maximizing is itself a function of market systems or is a human universal.

The question as to why it is that in certain systems not all the variables called for by the equation are present is another question entirely.

*Economic concepts and social theory.* The use of economic data and/or concepts in a theory does not in itself make what results an economic theory. An economic theory is one that relates economic concepts or variables to each other, usually within specified social, political, and cultural parameters. Economic concepts and data may also serve as inputs to a theory whose outputs are statements about social or political relationships, activities, etc., or about the relationship between economic concepts or data and social ones; but such a theory ought to be seen for what it is: a social theory that utilizes economic concepts and data. Keynes presented an economic theory; Marx utilized economic data and concepts to formulate a social theory. To lump both together as economic theory and then argue which kind of "economic theory" anthropology ought to have is to further confuse an already confused argument.

*Theories of primitive economics.* From the preceding it follows that the logical parallel of market theories for market societies are theories of primitive economics wherein both input and output can be stated in terms of economic concepts (preferably quantified) while social and cultural variables are considered as the parameters within which the equations are operative. A statement which related variations in the number of transactions in the kula ring to specific quantitative variations in the production of yams would represent a theory of Trobriand economics (though I am not holding my breath waiting for such a demonstration).

As Dalton points out (p. 72), attempts to deal with primitive economies in strictly economic terms have not proven fruitful. If this is necessarily the case, let us accept it and address ourselves to social and cultural questions, problems, and theories, utilizing economic data and relevant economic concepts. Let us remember, however, that such questions, problems, and theories are about society and/or culture and not, as Dalton seems to suggest, about primitive economics.

*Assumptions about the nature of motivation.* Market theories assume that individuals

will act in accordance with the demands of the market in order to maximize their financial returns, i.e., that they will economize. Clearly one cannot "economize" in the monetary sense in systems which have no all-purpose money. However, a good deal of the heat generated in the economic anthropology debate has centered on the question of whether the attempt to maximize certain returns is a universal. If so, it would be characteristic of participants in all types of economies in all social systems. Conversely, Dalton and others have suggested that economizing (maximizing—the two sometimes get confused) is itself a result of the characteristics of the market system as these ramify throughout those social structures in which this kind of economic system appears.

In either case the question has no direct, or operationally significant, bearing upon the questions of the applicability to primitive economies and societies of *market theories or concepts*. These questions, I have argued, can be answered on strictly pragmatic grounds, regardless of one's position in the maximizing argument. Nor do the answers to these questions support either position in the maximizing argument. The question as to whether or not all people maximize, then, must be dealt with independently of the other issues in the economic anthropology discussion.

Is a statement about maximizing one about the psychological state of an individual—"greed and self-aggrandizement" (Dalton p. 79)—or is it rather an assumption? If the latter, its value lies not in whether the statement is epistemologically true but rather whether or not it is intellectually and theoretically useful, i.e., whether or not it allows you to make certain predictions and orderings of the data which you could not otherwise make.

There is, of course, no question about assuming that participants in market systems attempt to maximize as they choose among alternatives. Dalton gives us the example of an American farmer making "economizing decisions" as he reaches a choice of which among several alternative crops to plant. Although Dalton does not say it, I am sure that he would agree that the farmer also considers technological and environmental parameters, though these too, in our system, could be reduced to monetary considerations (Dalton p. 66). In order to make his analysis comparable to the next step he takes, it is necessary for me to add that the American farmer must learn to economize, that is, to maximize a monetary return.

Dalton then goes on to contrast the American farmer with the Trobriander. A Trobriander, Dalton tells us (p. 67),

learns and follows the rules of *economy* in his society almost like an American learns and follows the rules of *language* in his. An American is born into an English-speaking culture. In no

sense does he "choose" to speak English because no real alternative is presented to him. So too, the Trobriander is born into a yam-growing economy. He does not "choose" to plant yams rather than broccoli. The question does not arise in this form, but rather in the form of how much of each of very few conventional crops to plant or how to apportion a given work day to several tasks.

This example, meant to show the absence of choice, reveals that in fact there is choice. The latter part of the above quotation, as well as the primary sources, indicates that a Trobriander must indeed make choices among alternatives, though they are certainly different kinds of alternatives than those which the American farmer must face. Dalton tells us (p. 67) that the Trobriander is operating in terms of economic, technological, and social constraints which lead him to carry out certain actions. These are exactly the same kinds of considerations that are dealt with by Barth (1966, 1967) in his analyses of decision-making among Norwegian fishermen, Fur household organization, and Kurdish inheritance practices. Such an analysis necessarily assumes that the subjects are maximizing. The trick, of course, is to determine what it is that they are maximizing, since it is not always a monetary return.

What we are faced with, then, is not the opposition which Dalton originally presented to us: that of the American farmer who makes economizing decisions versus the Trobriander who "learns and follows the rules of the economy." Rather what we must now consider is, on the one hand, the American farmer seeking to maximize a monetary return by balancing a number of financial, technological, and other constraints and rewards in arriving at his choice of one among a great number of alternative actions; and, on the other hand, the Trobriander, operating within a system of constraints and rewards of different kinds and choosing among far fewer alternatives, but nevertheless also maximizing something (status, prestige, rank, power, though not a monetary return).

Conceptually, then, the difference between the American farmer and the Trobriander does not lie in the fact that the former is greedy and self-aggrandizing while the latter, presumably, is not. Nor does it lie in the fact that the American farmer learns to make decisions while the Trobriander learns to follow a few simple rules of the economy. The difference between the two is rather in the *systems* of rewards and constraints within which each makes his decisions and the number of alternatives each has available.

Suppose we assume that individuals do not maximize. What are the alternative assumptions? One alternative, of course, is that decisions by individuals in non-market societies are completely random. If this were the case, then all of us should change our line of work, for there could be no

order to any human behavior. Another alternative is that individual actions are passive, and predetermined, responses to the imponderable workings of "the system." But "the system" as we know it consists of statements which represent abstractions generated from the observation of human behavior, and the latter is inseparable from decision-making. We are, therefore, right back where we started. The *assumption* of maximizing (something—not necessarily monetary returns) is intellectually and theoretically useful. If a theory were explicitly or implicitly raised upon any other assumption about the nature of individual motivation the result would be intellectual chaos or, at best, a social science very different from that which we now have.

I would maintain, further, that the assumption that individuals—all individuals—attempt to make decisions with the view to some form of maximum return is useful for another reason. This assumption permits us to translate statements at one level of abstraction into statements at another level, using the individual, maximizing, decision-maker to serve as the intervening variable. Economics, supposedly the least humanistic of the social sciences, is also the only one which explicitly has a position for the individual in its larger systemic theories. This permits the economist to make a statement at one level of abstraction (or level of aggregation, in his terminology) and trace out its effects at another; e.g., a change in the discount rate (national economy level) leads company managers (firm level) to maximize their profits by not borrowing money for further investment at the higher rates, which reduces the number of jobs at their disposal or the productivity of workers they already have and therefore will tend to hold their wages, all other things being equal, to a given level. This in turn means that the individual employees (consumer level) will have less of a disposable income, which in turn will affect demand for certain products, etc., etc.

In fact, anthropologists and other social scientists do this kind of analysis as well, though they often fail to be explicit about doing so (Homans 1964). Dalton, in fact, does implicitly assume that maximizing behavior occurs (even among primitives) when he deals with social-economic development. He tells us (p. 78, italics mine),

*Primitive* and peasant unwillingness to change production is frequently a sensible expression of their poverty and material insecurity. They cannot afford unsuccessful experiments.

On the following page he tells us that, "Anthropologists are concerned with minimizing the social costs of community transformation. . . ." He goes on to say that we can achieve this end by using

those powerful levers of new achievement which *the people themselves* [italics mine] perceive as desirable and which induce other positive changes—higher income through new economic and technological performance, and wider alternatives through education.

These three statements clearly assume that primitives, peasants, and anthropologists may be presented with alternatives to current practices, evaluate these alternatives, and choose among them in order to maximize some end.

To summarize: I believe that the controversy in economic anthropology has reached a point where it is useful to make some finer distinctions than have been made among the issues involved. The argument has centered on five issues which are conceptually distinct but which have been intermingled in the course of the argument. These are:

a) The applicability of economic concepts to primitive societies. It is clear that some economic concepts are applicable to such societies and others are not.

b) The applicability of market theories to primitive societies. It is clear, solely on operational grounds, that they are not.

c) The use of economic data to generate statements about society and culture. Anthropologists and other social scientists have been using such data, and rightly, for some time, although for one period in the history of our discipline this kind of data was ignored by many anthropological practitioners. The use of economic data, or even economic concepts, to generate and develop statements about society or culture does not transform such statements into economic analysis or economic theories.

d) Requirements for a theory of primitive economics. A theory of primitive economics must include not only inputs of economic data but also outputs stated in terms of relations among economic variables.

e) The universality of maximizing (economizing). In the narrow monetary sense, the concept of maximizing can only be useful in those situations and systems where the measurement of value is in terms of an all-purpose money. In the broader sense, however, we must assume that it does apply everywhere. If we assume otherwise we will be faced with chaos, not only within the confines of "economic anthropology" but in the discipline as a whole.

by CAROL F. SWARTOUT ☆

Long Beach, Calif., U.S.A. 1 VII 68

George Dalton's paper points up several serious problems in anthropology which have significance beyond the boundaries of economic anthropology. I cannot share Dalton's optimistic view that other branches of social-cultural anthropology have solved the problems of developing cross-culturally useful definitions and



theoretical models (p. 64). It is true that we do not use such blatantly ethnocentric concepts as "Christianity" and "democracy" in our analysis of primitive religious or political systems; but neither does formal economic theory imply that "capitalism" is the conceptual framework for analysis of primitive economies, rather simply that there should be some continuity between modern and primitive systems of economics.

The basic question can be broken down into two parts: (1) Can conceptual models developed to fit modern Western institutional arrangements be usefully, or validly, applied to primitive non-Western cultures? (2) Can a universalistic theory be developed which can adequately deal with the variability of the phenomena cross-culturally? A negative answer to the first question does not necessarily imply a negative answer to the second question.

Dalton argues cogently against the validity of applying Western economic theories to primitive and peasant societies, but he draws an illogical conclusion from his argument in assuming that the answer to the second question must also be "no." Dalton's argument appears to run that because there are several *kinds* of systems, there must be different kinds of analytic schemes. Conventional economic theory is not applicable to primitive systems because primitive systems are different in kind from modern economic systems. It does not follow, however, as Dalton implies (p. 66, that because conventional economic theory does not apply to primitive systems a universalistic theory of economics is not possible or desirable.

The key term in Dalton's argument is *kinds* of economies; for Dalton, the difference between modern and primitive systems is a matter of "kind" and not a matter of "degree." Given this assumption, then obviously, a universalistic theory of economics is logically impossible. Firth (1966: 14) and LeClair (1962: 1188) are arguing that the difference between modern and primitive systems is a matter of "degree" and not of "kind," and that one general theory can be developed which accommodates all of the variations.

Dalton's argument that we need different models for primitive, peasant, and modern economic systems is perfectly valid, but the fact that we use different models for different levels of complexity does not imply that these models cannot be linked together within a general theoretical framework. The real issue is not whether a general theory is possible or desirable, but whether the application of conventional economic theory is appropriate to primitive systems. One can accept that this is not a useful procedure without abandoning the idea of a universalistic theory.

One possible solution might be to deal with the phenomena in terms of a hierarchic arrangement such that attributes which occur at the lower levels of com-

plexity will be found repeated at all higher levels of organization, while the higher levels of organization will possess attributes never found at lower levels of organization. Thus, one cannot analyze economic behavior among the Bushmen in terms of "pricing mechanisms," but one can apply the analytic concepts of "gift-giving" or "reciprocity" in a modern complex society. Gift-exchanging is found at precisely the same level of social interaction in both modern and primitive societies, i.e., at the level of personal face-to-face relationships.

Thus, I agree with Dalton that much of conventional economic theory is not applicable to primitive systems, but I would argue that the concepts developed for primitive systems are applicable to modern complex systems. The problem is that one must make the application at the same level of social interaction in both societies. One cannot compare the economy of a band or a tribe with a nation-state, but one can compare systems of exchange within families, neighborhoods, and friendship circles cross-culturally.

The problem with general theory is that the proponents have started with the most complex instance and attempted to use this as a model for analyzing simpler instances. This approach is invalid. One must start with a model of the simplest instances and then build a theory which accommodates the more complex instances. You can't build a model of a cell by staring at a whole human being.

by LOUISE E. SWEET ☆

Winnipeg, Canada. 28 VII 68

This excellent survey of "economic anthropology," with its fine bibliography for handy reference, provides a thorough "refresher" exercise. More importantly, it offers stimulating challenges to anthropologists studying the attributes and processes of a variety of economic systems, their conceptual problems, and their efforts to offer "theories" of change or analyses and classifications of types. Dalton mildly scolds anthropologists for an unsophisticated use of "conventional economics" (primarily capitalist-market economic theory with scarcely any reference to or use of socialist economics). Let us hope that in some equivalent journal of economics he will scold capitalist economists for their unsophisticated approaches to non-capitalist-market based societies.

It is true that many words remain our blinders. Dalton has pinned down, albeit in a bland and non-polemical manner, both the faults of those anthropologists who would convert all sociocultural events to greedy, or at best reciprocal, economizing transactions and the faults of economists who work within conceptual constraints based on the study of economies of market-based, industrial nation-states. At the same time, however, he himself obscures certain

issues by avoiding some concepts and using others which, fashionable as they are, are to me euphemisms with ethnocentric derogatory implications. Terms such as "underdeveloped," "developed," and "modernization" obscure the increasingly destructive development and proliferation of industrial civilization: the most destructive proliferants are, it seems evident, particular Euro-American industrial polities. While Dalton does point to some examples of destruction (the "social cost," again a rather pallid term), his avoidance of such terms as "colonialism" and "economic imperialism," particularly in view of the fact that it has been bands, tribes, chiefdoms, archaic states, and peasantries under such impacts that anthropologists as ethnographers have studied, somewhat vitiates the force of his argument in Part III. Some anthropological archeologists, at least, have been far less hesitant to call an empire an empire, and to follow an old, scarcely read poet in delineating how "the Assyrian came down like the wolf on the fold."

I suggest that one generalization which Dalton offers here and elsewhere—that systems change only by stimulation from outside—is to be challenged. To judge from the position of the human species today, expansion and evolution are processes internal and inherent in the sociocultural mechanism of survival, and "adaptation" to external challenges is only one source of change. The dichotomy between primitive or peasant static and industrial dynamic systems is also, perhaps, a conceptual framework (or "model") as inhibiting to an understanding of the diversity and operation of cultures, in which their economies are subsystems, as the conventional economists' oversimplified dichotomy between "traditional" and "growing" economies.

I also have reservations about Dalton's discussion of peasantries. How can we divorce peasant economics from peasant culture if peasants are, as I would argue, primarily the food-producers in pre-industrial complex sociocultural systems? In Arabic-speaking countries, *fellah* (plural, *fellahiin*) is generally translated as "peasant"; it means, more literally, "sower of the land," with the connotation also of ruralness and that class and occupational status by which I have defined "peasant" above. Neither religion nor ethnic identity may be fundamentally as relevant as position in a structure—economic activity, social status, and sometimes political subordination to landlord and/or state. Peasant economy is a subsystem of the economy of a pre-industrial state, and *that* whole economy may include money, markets, craft industry, credit, and other specialized economic mechanisms.

Finally, Dalton's basic theme, that multiple theoretical frameworks are required because the very range of economic

systems that anthropology studies is so much greater than that of conventional economics, may be well taken at this point. He reinforces it by pointing out that, in fact, conventional economics has had to develop sets of theories as their range of interests and problems increased: economics is not all price theory. This has begun to happen in economic anthropology, certainly, and it is part of the trend toward ever increasing specialization and isolation of specialist from specialist, with greater and greater difficulty in maintaining the synthesizing, all-cultures, and species-wide or holistic focus of "traditional" anthropology. Therefore, it is amusing to me, and significant, that the last word in Dalton's essay occurs in a quotation from a work published originally in 1925 and is "evolution."

by STANISLAW TABACZYŃSKI ☆

Warsaw, Poland. 30 vi 68

George Dalton's work "Theoretical Issues in Economic Anthropology" meets the requirements of the present day and will certainly arouse lively interest among scientists employed in research on primitive societies. The economic theory of primitive society has up till now not been systematically elaborated, despite the fact that economic theories based on the capitalistic model and on the recently much-discussed feudal structure are obviously quite inadequate in respect to ancient epochs. An urgent need, therefore, is the creating of a scientific basis for economics for earlier periods of pre-statistic time (Wolff 1961: 847-48).

The study of Dalton's work suggests a number of thoughts to an archaeologist interested in the economic development of European primitive societies. It seems to me that in constructing a general theory of the economics of primitive societies one must take into account archaeological sources as well as ethnographic observa-

tions. The value of archaeological data lies primarily in the opportunity they provide for the study of economic processes in broad and clearly outlined chronological perspective. Careful examination of archaeological data permits a general characterization of the economic bases of development in primitive societies from the early Stone Age up to the feudal period.

Only long sequences of evolution, however, involving repeated phenomena and socioeconomic processes of long duration, are accessible to archaeological study. The dynamics of short-term changes cannot always be seen in archaeological data. Further, neither the finds nor the mediaeval written sources provide direct data for a full *quantitative* characterization of economic phenomena (Cipolla 1949: 4-5). This difficulty is not only a result of insufficiency of research. Its causes are to be sought in the very processes that have created the sources and in the factors that distort the picture acquired by investigation. Determining the degree of quantitative representative value of our data is likely to remain for a long time a controversial problem. What we can obtain from archaeological data, then, is a rather general macroscopic picture in *qualitative* categories, complemented by certain, only relative, quantitative indexes—"growth" vs. "decline," "casualness" vs. "universality," "large" vs. "small"—of the economic phenomena examined.

Should we accept that the task of an economic theory in every epoch is the determining of the rules governing production and distribution, both in the short and the long run (Kula 1962: 12-13), then it will have to be acknowledged that the sources and methods we dispose of today will not allow us to accomplish this task. We cannot yet formulate an economic theory of primitive societies on the basis of archaeological data alone. On the other

hand, ethnographic observation is bound to be one-sided, partly because it operates over short periods of time and lacks a sufficiently deep and continuous chronological perspective. In view of this, it is encouraging to observe the recent progress of anthropological, economic, and historical (including archaeological) studies in the field of research with which we are concerned. This progress is clearly manifest in the elaboration of a common language and the establishment of means of sharing scientific information, both of which contribute to a fruitful dialogue between representatives of various disciplines.

Research on remote periods of pre-statistic time presents particular difficulties. Overcoming these difficulties is an essential problem that now faces prehistory, on the one hand, and economic anthropology, on the other.

by ANDREW P. VAYDA ☆

New York, N.Y., U.S.A. 28 vi 68

Dalton continually talks about "the economy," but the closest he comes to defining it is when he refers to it as a "set of rules of social organization." If this is indeed intended as a definition, it comes no closer than other definitions (which I have criticized elsewhere [Vayda 1967]) to any adequate demarcation of the "economy" as a sphere of human activities, structures, or rules within the totality of human activities, structures, or rules. Does economic anthropology as Dalton conceives it have any distinctive subject matter or even any distinctive investigative techniques or theoretical framework? Can Dalton honestly say that in the study of primitive societies there is a place for something called "economic anthropology" as a subfield within social or cultural anthropology?

## Reply

by GEORGE DALTON

Several of the comments illuminate the psychology of polemics more than they clarify theoretical issues in economic anthropology. Accordingly, I shall deal first with the points of substance raised, and then with the polemics.

The following *points of substance* seem to demand discussion:

*Universal theory.* Those who think that universal theory in economic anthropology is achievable and desirable should do two things: (1) They should clarify what they mean by pointing to models (if, indeed, they exist) in other branches of anthropology or other *social sciences*, and then show the usefulness of such theories. The

point is that we attach different meanings to what a universal theory is. Swartout's question, "Can a universalistic theory be developed which can adequately deal with the variability of the phenomena cross-culturally?" does not make obvious sense to me. I do not know what such a theory looks like. Do any exist in social science? If so, where are they, and why are they a good thing? (2) They should go ahead and create such theory themselves and thereby convince those of us who are skeptical. In my article I quoted Firth as an advocate of universal theory. Firth has published a great deal of economic anthropology over 40 years without, I think, having produced such theory.

This semantic difficulty is a pervasive problem in economic anthropology. Each of the words, "capital," "market," "sur-

plus," and "money," means several complicated things in our own economy. Their counterparts in primitive economies are in some ways similar and in some ways different, yet we use the same words. A digging stick (like a machine in a U.S. factory) is "capital" in the sense that it is a tool used in a production process and is not consumed directly by people, as is food. It is not "capital" in the financial or wealth sense, that is, a piece of property having attached to its ownership shares of stock which can be bought and sold on stock markets and which yields its owners a money income. When an anthropologist calls a digging stick "capital," he is stressing its similarity to tools and machines in our own economy, but he leaves the differences unmentioned. I cannot agree with Strickon's remark that

"in all societies there are things (tools, land, etc.) which we may accurately assign to the category of 'capital goods.'" Even in our own economy, a sewing machine bought by a seamstress is a capital good, while the same machine bought by a housewife is a consumption good.

The fact that intelligent men can disagree—and disagree rather heatedly—over long periods of time almost certainly means there are ingrained semantic difficulties underlying part of their disagreement. They are attaching different meanings to the same words—not only to "universal theory," and "capital," but also to "applying economic theory." Some anthropologists think they are applying economic theory when they use the vocabulary of price theory to describe whatever transactions they observe in primitive economies. Instead of using ordinary language and emphasizing the folkviews involved, they use the language of price theory and draw an unwarranted analytical conclusion: e.g., instead of saying that a Trobriander gives yams to his sister's husband partly to fulfil an obligation to his closest female relative and partly in recognition of her rights to land he is using, they say the Trobriander is "maximizing prestige." That the meanings of universal theory and of other concepts are real sources of contention is made very clear in a recent symposium volume which considers the relevance of conventional economics to the processes and problems of economic development (Martin and Knapp 1967).<sup>19</sup>

*The social and cultural aspects of industrial capitalism.* Three of the respondents (Frankenberg, van Emst, and Grigor'ev) suggest that conventional economics is not sufficient for analyzing all the interesting features of Western economies. Economists would readily agree (see Lewis 1962), because they know that economics does not consider socio-economic processes and activities or even the minutiae of business-firm activity. Why else do Industrial Sociology and Business Administration exist as fields of study?

*Choice of primitive economies.* Schneider comments that

... Dalton, of course, asserts that such [primitive] economies are too small-scale for the range of choices to be comparable to those in our own economy.

I certainly do. One can divide production lines in U.S. industrial capitalism into two kinds: the overwhelming majority that have no counterparts in primitive economies (e.g., steel and automobile production), and those that do (e.g., farming,

fishing, and herding). For both sorts, the use of machinery and applied science create a range of technological choice that is incomparably greater than producing units have in primitive economies. The economic fact, moreover, that households in the U.S. economy receive income in the form of money and have market access to thousands of goods and services produced all over the country and the world means a range of consumer choice that is incomparably greater than the range of choice available to households in moneyless economies producing only a few goods.

*Maximizing in primitive economies.* Where anthropologists say that a person in a primitive economy (one without markets and Western money) "maximizes," I would substitute the phrases, "prefers more goods to fewer goods," "prefers one kind of good to another," or "prefers one kind of economic activity to another." Without money and pricing to compare real alternatives, there is no way of knowing that a specific choice yields the maximum. I agree with most of Strickon's comment, but not with the alternatives he poses: maximization or chaos. Choosing preferred goods or activities is not the same as maximizing. Again, anthropologists sometimes use the word maximizing to convey what I would call "ingenuity in exploiting the physical environment": knowing what is edible and what is not, the habits of game animals, etc. The Kalahari Desert pygmies' making the most of their environment with the simple technology they possess is not the same as an American firm's maximizing its profit.

*Economic anthropology and archeology.* I agree with Tabaczyński that there are mutual benefits to be had from economic anthropologists' considering archeological data and from archeologists using the data and theory of economic anthropology (as my colleague Stuart Streuver is doing, and also Colin Renfrew and Creighton Gabel).

*Community change begins with incursion from outside.* A persistent difficulty in creating theory in all branches of anthropology stems from the fact that the real-world communities anthropologists study number in the thousands, are on all the continents, and have been studied at different points in time. It is such diversity and complexity, both in their empirical universe and in the many aspects of society they study, that leads anthropologists inevitably to create classifications, groupings, typologies, and subsets (matrilineal/patrilineal; acephalous/centralized; primitive/peasant; hunters/agriculturalists/pastoralists), and then to try to state similarities and differences among groupings so as to draw interesting analytical conclusions.

I have read what must be several dozen case studies of social and economic change in village-level communities, with three microdevelopment (i.e., modernization) questions in mind: (1) What features of

traditional economy and society make for receptivity or resistance to modern sorts of innovations? (2) Once a community begins to use modern technology and expands commercial production, how and why do traditional social organization and culture change? (3) What have been the time dimensions of socio-economic change, and what sequential events characterize change over time?

For the cases I have read, I think it is true that socio-economic change is very frequently (if not invariably) initiated from outside the village-level community; and that the answers to all three questions are affected by the kinds of initial incursions that take place. We need typologies here because there are so many cases in so many parts of the world reported over such long periods of time. I would suggest, moreover, that studying pre-industrial compared to post-industrial Europe helps to understand pre-colonial compared to post-colonial Africa and Asia. The formation of nation-states, industrialization, and the creation of national-market integration are watershed changes (see Adelman and Morris 1967). Pre-industrial Europe, like pre-colonial Africa, was not static; but the kinds of change experienced—war, conquest, political subjugation, plague, famine, acquiring a few new material items from abroad, new religions—affected societies and economies differently from the radical structural changes generated by industrialism, national-market integration, and the formation of nation-states of the modern sort.

*Polanyi on "householding."* I agree with Riegelhaupt that Polanyi's discussion of householding is really about peasantry. His remarks are brief, however, and (to his later regret) he did not develop his ideas on householding at length in *Trade and Market in the Early Empires*.

*Marx and economic anthropology.* Grigor'ev and Frankenberg suggest that several writers on economic anthropology (including myself) are being unnecessarily original; that what we say about the way economy relates to social organization is quite close to Marx's views, and we are not giving Marx's writings sufficient prominence in economic anthropology. I think they are probably right, not only for Marx, but also for Maine, Tönnies, Weber, and Bloch. A history of economic anthropology is yet to be written which shows how the leading ideas of these earlier writers on economy and society influenced the theoretical framework of economic anthropology that has been created since Malinowski.

*Economy and society.* I agree with Blacking that my phrasing is misleading. Saying that "economic organization in primitive or subsistence economies is frequently an expression of kinship, religious, or political relationships" attributes primacy to the social relationships and a subordinate,

<sup>19</sup> The volume begins with the provocative article by Dudley Seers that brought about the symposium, "The Limitations of the Special Case." See also Myrdal (1957), Hagen (1962), Sawyer (1951), Singer (1950), and Rostow (1963).

derivative character to the economic arrangements. I should prefer not to attribute primacy to either. I think Godelier's (1965) phrasing, quoted in Kowalewski's comment, and Evans-Pritchards' statement (1954: vii) are good ways of stating the inextricable connections between economy and society and the social complexity in which economic arrangements are embedded in primitive societies.

*Is there an economy separate from society? Should economic anthropology exist?*<sup>20</sup> Vayda argues that for the communities anthropologists study, it is extremely difficult to define and demarcate the "economy," unless one makes the definition and demarcation so broad as to include virtually all activities, relationships, and institutions that anthropologists are interested in anyway; that kinship, religion, and other spheres of social organization and culture are so intimately bound up with land, labor, and material exchanges that one cannot say unambiguously what is "economic" and what is not. This is particularly so in what some anthropologists call the prestige sphere—bridewealth, potlatch, warfare, bloodwealth, fines, etc.—which involves exchanges and payments as well as honor, prerogatives, status, and social relationships.

In short, Vayda puts three related questions: (1) Can one define the economy in such fashion as to demarcate it from the rest of society? (2) Does economic anthropology have any distinctive subject matter, techniques of analysis, or theoretical framework? (3) Is it worthwhile retaining the special subfield called economic anthropology?

I readily agree that to analyze, say, land acquisition, usage, and transfer in primitive economies, one must bring in kinship or tribal affiliation; and to show the functions of treasure items or primitive money, one must bring in marriage and perhaps conflict resolution. Of course, the reverse is also true: if one had a special analytical interest in kinship, marriage, or politics, he would have to bring in land and other material transactions. This structured interleaving of what we separate out in Western societies as economic, political, religious, and social is a salient characteristic of organic or *Gemeinschaft* systems. In the pre-industrial societies studied by anthropologists and historians, many important institutions cannot be classified simply as economic, or social, or political: feudalism, peasantry, slavery, caste are socio-economic categories. To analyze them as though they were exclusively economic would be distorting. This does not mean, however, that there is no such thing as an economy in such societies or that economic anthropology is pointless. It

means that to analyze economic organization and performance sensibly one must also analyze social organization, technology, and ecology.

I appreciate but do not share Vayda's underlying qualm: that unless the economy can be defined and demarcated as a distinct entity apart from the rest of society and culture, the special analytical concern with material organization and performance we call economic anthropology is not worth pursuing. To answer his questions directly:

An economy is a set of institutionalized activities which combine natural resources, human labor, and technology to acquire, produce, and distribute material goods and specialist services, in structured, repetitive fashion. Even in U.S. capitalism, the economy cannot be demarcated from the rest of society in watertight fashion, because material goods or money are used to express social relationships (e.g., gifts at marriage as an expression of kinship or friendship) and, more importantly with us, because of the several ways in which government (especially since the New Deal) affects the economy and households (taxes, welfare expenditures, drafting manpower for war, etc.). In our own economy, however, the private market sector is still so large, important, and pervasive that economists hive off its organization, performance, transactions, and measurement as their special analytical interest. They realize that in so doing they ignore many socio-economic activities. These, however, have come to be studied either by the occasional economist with "institutional" interests (the Veblens and Galbraiths), or by specialists in other subjects such as Business Administration, Industrial Sociology, or by those who do research on the psychology of consumer purchases.

In primitive societies, where there is no market sector (or only a petty one confined to a few produced goods), some functions that markets perform in U.S. economy are performed instead by kinship, religion, polity, etc. With us, labor for production is recruited principally through labor markets; in primitive societies principally through the nuclear or extended family.

Economic anthropology is coming to have a distinctive subject matter and theoretical framework and is worth retaining as a subfield within cultural or social anthropology. Just as economics delineates principles of economic organization for national capitalist and communist economies, so economic anthropology delineates principles of socio-economic organization for small-scale primitive and peasant economies. I should hope that those who specialize in economic anthropology would come to share my view that its distinctive subject matter comprises: (a) the organization and performance of pre-industrial economies as these relate to

social organization and culture; and (b) past and present processes of socio-economic change, growth, and development, with particular emphasis on modernization and the integration of village communities into national economies and societies. Further, I should hope that, in their analyses of both traditional structures and dynamic processes, economic anthropologists would draw on empirical data from the other social sciences and from history. Comparative analysis is important to economic anthropologists whether they are interested in complicated institutions found widely—slavery, feudalism, peasantry—or in socio-economic devices whose organization and functions vary with underlying structure—money, markets, external trade.

I shall not attempt to summarize here what I have written over the last ten years on the theoretical framework of economic anthropology, but I should like to give the reasons why I think anthropologists have a distinctive contribution to make to that branch of comparative economic systems which is economic anthropology. Anthropology combines several special characteristics which equip anthropologists to undertake socio-economic analysis in a way both different from that of other social scientists and especially productive: (1) its traditional focus of interest on the small-scale societies of the village or tribal segment; (2) its technique of fieldwork immersion in the life of the community for a sufficient length of time for the anthropologist to feel and understand its workings; (3) its broad interests in all the principal segments of social life—family, polity, economy, religion, etc.; (4) its special concern with the interaction of the component activities, institutions, and relationships of society and culture. What Vayda (like, of course, Polanyi) underscores is anthropologists' inability to demarcate purely economic principles in the organically structured societies they study. Socio-economic principles can be delineated, however, and to do so requires what only anthropologists are professionally equipped to do: to show, as do Douglas (1962) and Epstein (1962), how culture and society, economy, technology, and ecology, and polity and religion mutually affect one another both in static institutional structure and in change.

Now, as to the *polemics*:

I can only account for the widely disparate assessments of my article (compare, e.g., the comments of Arensberg and Salisbury) and the scornful language used by several of the writers (particularly Schneider and Nash) by considering the psychological difficulties that obstruct the settling of theoretical issues in the social sciences by reason, logic, and dispassionate analysis. One's conceptual framework

<sup>20</sup> I consider some of the questions Vayda raises in several articles still in press (see Dalton 1968c, 1969a, b, c).

apparently, can become a sort of professional religion, the questioning of which evokes rage. Theoretical disputes therefore resemble theological battles. One doesn't listen to heretics, one merely slays them.

The slaying is done in several ways. One way is simply to dismiss the work (without further argument, analysis, or proof) as wrong, muddled, or trivial, as Nash does:

The institutional analysis of Polanyi is rather crude and ad hoc. His principles of exchange are descriptive of some societies, but have little analytical value. As Dalton's Table 1 indicates, the principles lead to mechanical, schematic, and static understandings at a superficial level. What Polanyi has to offer social and economic anthropology has already been absorbed and superseded. . . .

Elsewhere, Nash (1967: 250) has simply dismissed, in very strong language, issues that anthropologists from Malinowski, Firth, and Herskovits to Bohannan and Sahlins have thought worthy of serious analysis:

From the beginning the substantivists (as exemplified in the justly famous works of Polanyi and others) were heroically muddled and in error. It is a tribute to the maturity of economic anthropology that we [*sic*] have been able to find precisely in what the error consisted in the short space of six years. The paper . . . written by Cook [1966] when he was a graduate student neatly disposes of the controversy. I did not think it necessary [in my book] to regale readers with the history of error. [In social science] . . . it is virtually impossible to down a poor, useless, or obfuscating hypothesis, and I expect that the next generation of creators of high-level confusion will resurrect, in one guise or another, the substantive view of the economy.

Nash has never written on these issues that he condemns so vigorously. For him to make pontifical judgements in such abusive language is irresponsible. I suspect that he again finds it necessary to deprecate what he does not understand (see Dalton 1968*b*).

Another method of slaying is to attribute reprehensible qualities, base motives, and generalized sneakiness, as Schneider does in referring to "Dalton's uncompromising approach" and in saying that

[Dalton] is addicted to dogmatic pronouncements . . . he misrepresents his critics. . . . Dalton argues by means of ex cathedra assertions from a dogmatic position which assumes his critics to be ignorant . . . he ridicules the search for general laws. . . . [Dalton's position] is the ultimate in cultural-relativism, or particularism, and is utterly contrary to a scientific approach.

Another slaying device is less obvious. It is a Gresham's Law of Ideas in which the detection of error of any sort is taken as sufficient reason to condemn everything the man writes. If a Karl Marx argues something silly (say, a labor theory of value), some Anglo-American economists, already disliking Marx, use this as an

excuse to dismiss his entire corpus of work. If, on the other hand, a David Ricardo argues something silly (say, a labor theory of value), they do not dismiss his theories of rent or international trade; they like Ricardo. In theoretical disputes, it seems, we overkill only those we already dislike: says Salisbury, "My own disagreement with Dalton is virtually complete."

There is, moreover, the extreme difficulty of reading one's critics with the same concentration, absorption, and care with which one reads one's supporters. One reads critics for their weak, not their strong points; for ammunition, not enlightenment. It seems as though we all have deep grooves in our heads shaped by the conceptual categories we find most congenial. When we read our opponents we sometimes perform an instant translation in our heads, a transformation of their words into our own categorical grooves. The result is to attribute positions to our opponents rather different from the ones they are actually arguing, and by so doing, to create straw men. For example, Schneider remarks that

. . . it is anachronistic to see Dalton categorizing the world as consisting of Western (capitalist economies) and "primitives" (all the rest).

My paper has a section on peasant economies; it shows awareness that Communist economies exist and also that underdeveloped national economies and changing village-level economies exist. Schneider goes on to argue that

to him [Dalton] economy is merely the system of distribution of wealth and society is the whole range of dependency relations in a community, all of which except in capitalist society, are altruistic. . . . Dalton gives the impression that choice is a luxury, or a component only of capitalist societies—that in non-Western economies rules take care of the distribution of wealth and choice is unnecessary. (His additional defense [*sic*], that wealth is so scarce that there is not much to exercise choice about, is, I think, simply a misconception. . . .) I suspect that cattle would be rejected [by Dalton] as money because they don't look like money. . . .

Here he puts words in my mouth that are extremely distasteful to me. I hold no such views, nor does anything I say in print suggest that I do.

Nash, in saying that

For an essay entitled "Theoretical Issues in Economic Anthropology," it is surprising how few such issues are mentioned or discussed here. (Compare, for example, the range of issues raised in Firth [1967], Belshaw [1965], or Nash [1966]).

is comparing this single journal article with three books; I invite comparison with the dozen or so journal articles on economic anthropology I have published. His statement that

Dalton is concerned with a single issue—that of the relevance of contemporary economic concepts and modes of analysis to primitive and peasant economies—and it is one that was probably not even meaningful in the form which Polanyi first raised it.

is a bit of "intellectual slippage" on Nash's part. My article is also about the distinctions between primitive and peasant, and between static and dynamic economies; and between structure and performance of economies. There is also some discussion of the connections between economic and social structure and of the contrast between perceiving the economy as what individual behaviors do and seeing it as a set of rules of social organization. There are sections on the difficulties of contriving universal theory, the importance of markets as a differentiating feature among economies, the crucial constraining role of small size and primitive technology in determining economic organization and performance, and the special redistributive and administered trade sectors where centralized government is present. Several other matters are discussed as well.

Polanyi, moreover, was not the first to raise the issue of the relevance of economics to economic anthropology. Aside from Malinowski (1922), Goodfellow (1939: Chap. 1), and Firth (1939: Chap. 1), Herskovits devoted an entire chapter to it in his *The Economic Life of Primitive Peoples* (1940) and defended his views in his exchange with Frank Knight (1941). Polanyi responded to the issues briefly in Chapter 4 and its appendix of *The Great Transformation* (1944), and in "Our Obsolete Market Mentality" (1947), and at length in Chapters 5, 12, and 13 of *Trade and Market in the Early Empires* (1957).

After simply dismissing Polanyi's work as "crude and ad hoc," having "little analytical value," and leading to such "mechanical, schematic, and static understandings at a superficial level," Nash concludes rather surprisingly by suggesting that Polanyi has contributed something (although it has already been absorbed and superseded) and by hinting that he is one of the founders of the science. Polanyi's book on economic anthropology was published in 1957. What has superseded it? How can anyone producing such "crude" work having so "little analytical value" still be regarded as one of the "founders of the science" who has written "justly famous works"? Nash not only overkills, he also contradicts himself, and his remarks are mere assertion throughout. I wish, moreover, that I could agree with Nash that Polanyi's work has been absorbed. There is so much evidence to the contrary—that essential points of his are misunderstood—that in 1968 I published a collection of his essays most pertinent to economic anthropology together with an expository

Introduction (Dalton 1968a). My purpose was to bring to the attention of anthropologists the fact that Polanyi has written more of interest to economic anthropology than just his chapters in *Trade and Market in the Early Empires*.

As an example of the misunderstanding of Polanyi's work, I cite from the comment by Salisbury "... Polanyi's 1957 classification of economies into three types is useful. . . ." Reciprocity, redistribution and (market) exchange are not designations of entire economies, but rather of modes of transaction, or what Polanyi calls "patterns of integration." There is no economy which is organized exclusively by just one transactional mode. Moreover, Polanyi's principles are not merely of exchange, as Nash says, but of production

processes as well. As Polanyi (1960:331) has said,

These patterns [of integration—reciprocity, redistribution, and market exchange] do not—and this should be stressed—supply us with a classification of economic systems as a whole; rather the coexistence of patterns notably of reciprocity and redistribution is common. Also markets, which do not integrate the economy, may fit into either pattern. And any of the patterns may predominate, reflect the movements through which land, labor, and the production and distribution of food are merged into the economy. But other patterns may obtain alongside the dominant one in the various sectors of the economy and at varying levels of its organization.

I decided not to write a rejoinder to LeClair (1962) and Burling (1962) in the hope that the misunderstandings they contain would be cleared up over time. I think that rejoinders are less likely to enhance understanding than fresh analysis of real-world data. Reading Cook (1966), who is both caustic and adamant, however, induced me to try to clarify the issues and clear up the misunderstandings. I am pleased that several of the respondents thought the effort worthwhile. I shall now get on with the great deal of substantive work there is yet to do in both branches of economic anthropology—traditional economies and socioeconomic change and development.

## References Cited

- ADELMAN, IRMA, and CYNTHIA TAFT MORRIS. 1965. Factor analysis of the interrelationship between social and political variables and per capita gross national product. *The Quarterly Journal of Economics* 89: 555-78.
- . 1967. *Society, politics, and economic development*. Baltimore: Johns Hopkins University Press.
- ALLAN, W. 1965. *The African husbandman*. London and Edinburgh: Oliver and Boyd. [RF☆]
- ALTHUSSER, LOUIS. 1965. *Pour Marx*. Paris: Maspéro. [ZMK☆]
- ARENSBERG, CONRAD M. 1937. *The Irish countryman*. New York: Macmillan. [RF☆]
- ARENSBERG, CONRAD M., and ARTHUR H. NIEHOFF. 1964. *Introducing social change*. Chicago: Aldine.
- ARMSTRONG, W. E. 1924. Rossel Island money: a unique monetary system. *Economic Journal* 34:423-29.
- . 1928. *Rossel Island*. Cambridge: University Press.
- ARNOLD, ROSEMARY. 1957a. "A port of trade: Whydah on the Guinea coast," in *Trade and market in the early empires*. Edited by K. Polanyi, C. M. Arensberg, and H. W. Pearson. Glencoe: The Free Press.
- . 1957b. "Separation of trade and market: great market of Whydah," in *Trade and market in the early empires*. Edited by K. Polanyi, C. M. Arensberg, and H. W. Pearson. Glencoe: The Free Press.
- BALANDIER, GEORGES. 1956. "Le contexte socio-culturel et le coût social du progrès," in *Le "Tiers-Monde": Sous-développement et développement*. Paris: Presses Universitaires de France. [ZMK☆]
- . 1958. "Sociologie des régions sous-développées," in *Traité de sociologie*, vol. 1. Edited by G. Gurvitch. Paris: Presses Universitaires de France. [ZMK☆]
- BARAN, PAUL A. 1957. *The political economy of growth*. New York: Monthly Review Press; London: Calder. [RF, ZMK☆]
- BARTH, FREDRIK. 1963. *The role of the entrepreneur in social change in Northern Norway*. Bergen, Oslo: Norwegian Universities Press. [RF☆]
- . 1964. "Capital, investment and the social structure of a pastoral nomad group in South Persia," in *Capital, saving, and credit in peasant societies*. Edited by R. Firth and B. Yamey. Chicago: Aldine. [HKS☆]
- . 1965. Economic spheres in Darfur. Paper presented at ASA Conference on Economic Anthropology, Oxford, 1965. [EC☆]
- . 1966. *Models of social organization*. Royal Anthropological Institute Occasional Paper no. 23. [AS☆]
- . 1967. On the study of social change. *American Anthropologist* 69:661-69. [AS☆]
- BEATTIE, J. H. M. 1964. Bunyoro: An African feudalism? *Journal of African History* 5:25-36.
- BELSHAW, C. S. 1949. Economic aspects of culture contact in Eastern Melanesia. Unpublished Ph.D. thesis, University of London. [RF☆]
- . 1965. *Traditional exchange and modern markets*. Englewood Cliffs: Prentice-Hall.
- BENNETT, H. S. 1962. *Life on the English manor, 1150-1400*. Cambridge: University Press.
- BESTOR, A. E., JR. 1950. *Backwoods utopias*. Philadelphia: University of Pennsylvania Press.
- BIEBUYCK, DANIEL. Editor. 1963. *African agrarian systems*. London: Oxford University Press.
- BISHOP, CLAIRE. 1950. *All things common*. New York: Harper.
- BLOCH, MARC. 1961. *Feudal society*. London: Routledge & Kegan Paul.
- . 1966. *French rural history*. London: Routledge & Kegan Paul.
- BOAS, FRANZ. 1897. "The social organization and the secret societies of the Kwakiutl Indians," in *Report of the U.S. National Museum for 1895*. Washington, D.C.
- BOEKE, J. H. 1942. *The structure of Netherlands Indian economy*. New York: Institute of Pacific Relations.
- BOHANNAN, PAUL. 1954. *Tiv farm and settlement*. London: H.M.S.O.
- . 1957. *Justice and judgment among the Tiv*. New York: Oxford University Press.
- . 1959. The impact of money on an African subsistence economy. *Journal of Economic History* 19:491-503.
- BOHANNAN, PAUL and LAURA BOHANNAN. 1968. *Tiv economy*. Evanston: Northwestern University Press.
- BOHANNAN, PAUL, and GEORGE DALTON. 1965. "Introduction," in *Markets in Africa*. New York: Natural History Press.
- BOULDING, KENNETH. 1957. The Parsonian approach to economics. *Kyklos* 10:317-19.
- BOWEN, I. 1967. *Seven lectures on manpower planning, with special reference to Thailand*. Working Papers in Asian Studies 2. Nedlands: Center for Asian Studies, The University of Western Australia. [RR☆]
- BROKENSHA, DAVID W. 1966. *Social change at Larteh, Ghana*. Oxford: Clarendon Press.
- BURLING, ROBBINS. 1962. Maximization theories and the study of economic anthropology. *American Anthropologist* 64:802-21.
- CANCIAN, FRANK. 1966. Maximization as norm, strategy, and theory: A comment on programmatic statements in economic anthropology. *American Anthropologist* 68:465-469. [PKM☆]
- CARR, E. H. 1951. *The new society*. London: Macmillan.
- CHAMBERLAIN, E. H. 1933. *The theory of monopolistic competition*. Cambridge: Harvard University Press.
- CHAYANOV, A. V. 1966. *The theory of peasant economy*. Homewood: Irwin. (First published in Russian, in 1925.)
- CHEVALIER, FRANCOIS. 1963. *Land and society in colonial Mexico*. Berkeley: University of California Press.
- CIPOLLA, C. M. 1949. L'économie politique au secours de l'histoire. *Annales, Economies, Sociétés, Civilisations* 1:4. [ST☆]
- CLOWER, R., G. DALTON, M. HARWITZ, and A. A. WALTERS. 1966. *Growth without development: An economic survey of Liberia*. Evanston: Northwestern University Press.
- COLSON, ELIZABETH. 1949. Assimilation of an American Indian group. *Human Problems in British Central Africa (Rhodes-Livingstone Journal)* 5:1-13.
- COOK, SCOTT. 1966. The obsolete "anti-market" mentality: A critique of the substantive approach to economic anthropology. *American Anthropologist* 68:323-45.
- DALTON, GEORGE. 1960. A note of clarification on economic surplus. *American Anthropologist* 62:483-90.
- . 1961. Economic theory primitive and society. *American Anthropologist* 63:1-25.
- . 1962. Traditional production in primitive African economies. *The Quarterly Journal of Economics* 76:360-78.
- . 1963. Economic surplus, once again. *American Anthropologist* 65:389-94.
- . 1964. The development of subsistence and peasant economies in Africa. *International Social Science Journal* 16:378-89.
- . 1965a. Primitive money. *American Anthropologist* 67:44-65.
- . 1965b. "Primitive, archaic, and modern economies: Karl Polanyi's contribution to economic anthropology and comparative economy." *Proceedings of the 1965 Annual Spring Symposium of the American Ethnological Society*. Seattle: University of Washington Press.
- . 1965c. History, politics, and economic development in Liberia. *Journal of Economic History* 25:569-91.
- . 1966. Bridewealth versus brideprice. *American Anthropologist* 68:732-37.
- . 1967. "Bibliographical essay," in *Tribal and peasant economies: Readings in economic*

- anthropology. New York: Natural History Press.
- . 1968a. *Primitive, archaic, and modern economies: Essays of Karl Polanyi*. New York: Natural History Press.
- . 1968b. Review of: *Primitive and peasant economic systems*, by Manning Nash (San Francisco: Chandler, 1966). *American Anthropologist* 70:368-69.
- . 1968c. Economics, economic development, and economic anthropology. *The Journal of Economic Issues*. In press.
- . 1969a. "Economics, anthropology, and economic anthropology," in *Anthropology and related disciplines*. Edited by Otto von Mering. Pittsburgh: University of Pittsburgh Press. In press.
- . 1969b. "Traditional economic systems," in *The African experience*. Edited by John Paden and Edward Soja. Evanston: Northwestern University Press. In press.
- . 1969c. "The economic system," in *A handbook of method in cultural anthropology*. Edited by R. Naroll and R. Cohen. New York: Doubleday. In press.
- DANDEKAR, V. M. 1962. Economic theory and agrarian reform. *Oxford Economic Papers* 14: 69-80. [JFR ☆]
- DEANE, PHYLLIS. 1953. *Colonial social accounting*. Cambridge: University Press.
- DOMAR, EVSEY D. 1957. *Essays in the theory of economic growth*. New York: Oxford University Press.
- DOUGLAS, MARY. 1958. Raffia cloth distribution in the Lele economy. *Africa* 28:109-22.
- . 1962. "Lele economy compared with the Bushong," in *Markets in Africa*. Edited by P. J. Bohannan and G. Dalton. Evanston: Northwestern University Press.
- . 1965. "The Lele—resistance to change," in *Markets in Africa*. Edited by P. J. Bohannan and G. Dalton. New York: Natural History Press.
- DOBB, MAURICE. 1946. *Studies in the development of capitalism*. London: Routledge and Kegan Paul. [RF ☆]
- EPSTEIN, T. SCARLETT. 1962. *Economic development and social change in South India*. Manchester: Manchester University Press.
- ERASMUS, CHARLES J. 1961. *Man takes control*. Minneapolis: University of Minnesota Press.
- EVANS-PRITCHARD, E. E. 1954. "Introduction," in *The gift*, by Marcel Mauss. Glencoe: The Free Press.
- FALLERS, LLOYD A. 1961. Are African cultivators to be called "peasants"? *CURRENT ANTHROPOLOGY* 2:108-10.
- FALS BORDA, ORLANDO. 1965. "Violence and the break-up of tradition in Colombia," in *Obstacles to change in Latin America*. Edited by C. Veliz. London: Oxford University Press. [ZMK ☆]
- FANON, FRANTZ. 1959. *L'an V de la révolution algérienne*. Paris: Maspero. [ZMK ☆]
- . 1961. *Les damnés de la terre*. Paris: Maspero. [ZMK ☆]
- . 1965. *The wretched of the earth*. London: MacGibbon and Kee. [RF ☆]
- FIRTH, RAYMOND. 1929. *Primitive economics of the New Zealand Maori*. Wellington: R. E. Owen, Government Printer.
- . 1939. *Primitive Polynesian economy*. London: Routledge & Kegan Paul.
- . 1946. *Malay fisherman: Their peasant economy*. London: Routledge & Kegan Paul.
- . 1951. *The elements of social organization*. London: Watts.
- . 1957. "The place of Malinowski in the history of economic anthropology," in *Man and culture: An evaluation of the work of Bronislaw Malinowski*. Edited by R. Firth. New York: Harper Torchbooks.
- . 1958. "Work and wealth of primitive communities," in *Human types*. Revised edition. New York: Mentor Books.
- . 1959. *Social change in Tikopia*. London: George Allen and Unwin.
- . 1964a. "Capital, saving, and credit in peasant societies: A viewpoint from economic anthropology," in *Capital, saving, and credit in peasant societies*. Edited by R. Firth and B. Yamey. Chicago: Aldine.
- . 1964b. *Essays on social organization and values*. London: Athlone Press. [HKS ☆]
- . 1965. Review of: *Kapauku Papuan economy*, by L. Pospisil (Yale University Publication in Anthropology no. V). *American Anthropologist* 67:122-25.
- . 1966. Revised edition. *Primitive Polynesian economy*. London: Routledge & Kegan Paul.
- . Editor. 1967. *Themes in economic anthropology*. A.S.A. Monograph no. 6. London: Tavistock. [RF, MN ☆]
- FIRTH, RAYMOND, and BASIL YAMEY. Editors. 1964. *Capital, saving, and credit in peasant societies*. Chicago: Aldine.
- FORMAN, SHEPARD, and JOYCE F. RIEGELHAUPT. 1968. Market place, marketing system and the theory of peasant economy. Paper presented at the annual meetings of the American Anthropological Association, Seattle, Washington, November, 1968. [JFR ☆]
- FORTES, M., and E. E. EVANS-PRITCHARD. 1940. *African political systems*. London: Oxford University Press.
- FOSTER, G. M. 1965. Peasant society and the image of the limited good. *American Anthropologist* 67:293-315. [RFS ☆]
- FRANK, ANDREW GUNDER. 1966. Sur le problème indien. *Partisans* 26-27. [ZMK ☆]
- . 1967. *Capitalism and underdevelopment in Latin America*. New York: Monthly Review Press. [ZMK, JFR ☆]
- FRANKEL, S. H. 1955. *The economic impact on under-developed societies*. Cambridge: Harvard University Press.
- FRANKENBERG, RONALD. 1967. "Economic anthropology: One anthropologist's view," in *Themes in economic anthropology*. Edited by R. Firth. London: Tavistock. [RF, HKS ☆]
- FRIEDMAN, MILTON. 1962. *Price theory: A provisional text*. Chicago: Aldine. [MF ☆]
- FROMM, ERICH. 1941. *Escape from freedom*. New York: Rinehart.
- FUSFELD, DANIEL B. 1957. "Economic theory misplaced: Livelihood in primitive society," in *Trade and market in the early empires*. Edited by K. Polanyi, C. M. Arensberg, and H. W. Pearson. Glencoe: The Free Press.
- GEERTZ, CLIFFORD. 1962. "Social change and economic modernization in two Indonesian towns: A case in point," in *On the theory of social change*. Edited by Everett E. Hagen. Homewood: Dörsey Press.
- GEORGESCU-ROGEN, N. 1960. Economic theory and agrarian economics. *Oxford Economic Papers* 12:1-40. [JFR ☆]
- GERSHENKRON, ALEXANDER. 1954. Social attitudes, entrepreneurship, and economic development. *International Social Science Journal* 6: 252-58.
- . 1962. *Economic backwardness in historical perspective*. Cambridge: The Belknap Press of Harvard University Press.
- GODELIER, MAURICE. 1965. Objet et méthode de l'anthropologie économique. *L'Homme* 5(2).
- . 1966a. *Rationalité et irrationalité en économie*. Paris: François Maspero. [RF ☆]
- . 1966b. Système, structure et contradiction dans Le Capital. *Les Temps Modernes* 246. [ZMK ☆]
- GOLDSCHMIDT, W., et al. 1965. Variation and adaptability of culture: a symposium. *American Anthropologist* 67:400-47. [RFS ☆]
- GONZALEZ CASANOVA, PABLO. 1964. Société plurale: colonialisme interne et développement. *Tiers-Monde* 5 (18). [ZMK ☆]
- . 1965. *La democracia en México*. México: Era. [ZMK ☆]
- GLUCKMAN, MAX, and I. G. CUNNINGHAM. 1962. "Foreword," in *Politics of the kula ring*, by J. P. Singh Uberoi. Manchester: Manchester University Press.
- GOODENOUGH, WARD HUNT. 1963. *Co-operation in change*. New York: Russell Sage Foundation.
- GOODFELLOW, D. M. 1939. *Principles of economic sociology*. London: Routledge.
- GOODY, JACK. 1963. Feudalism in Africa? *Journal of African History* 4:1-18.
- GOUGH, KATHLEEN. 1968. Anthropology: Child of imperialism. *Monthly Review* 19 (11): 12-25. [RF ☆]
- GRAY, ROBERT F. 1960. Sonjo bride-price and the question of African "wife purchase." *American Anthropologist* 62:34-57.
- GROSSMAN, GREGORY. 1967. *Economic systems*. Englewood Cliffs: Prentice-Hall.
- GRUCHY, ALLAN G. 1966. *Comparative economic systems*. Boston: Houghton Mifflin.
- GULLIVER, P. H. 1965. "The Arusha—economic and social change," in *Markets in Africa*. Edited by P. J. Bohannan and G. Dalton. New York: Natural History Press.
- HAGEN, EVERETT E. 1962. *On the theory of social change: How economic growth begins*. Homewood: Dorsey Press.
- HARRIS, MARVIN. 1959. The economy has no surplus? *American Anthropologist* 61:185-99.
- HARROD, R. F. 1952. "An essay in dynamic theory," in *Economic essays*. New York: Harcourt, Brace.
- HART, C. W. M., and A. R. PILLING. 1960. *The Tiwi of North Australia*. New York: Holt, Rinehart and Winston. [HKS ☆]
- HERSKOVITS, MELVILLE J. 1940. "Anthropology and economics," in *The economic life of primitive peoples*. New York: Knopf.
- . 1941. Economics and anthropology: A rejoinder. *Journal of Political Economy*. 49:269-78. (Reprinted in *Economic anthropology*, by M. J. Herskovits [New York: Knopf, 1952].)
- . 1952. Revised edition. *Economic anthropology*. New York: Knopf.
- HILL, POLLY. 1963. *Migrant cocoa-farmers of southern Ghana*. Cambridge: Cambridge University Press.
- . 1965. *A plea for indigenous economics: The West African example*. Working Papers of the Economic Development Institute 5. Ibadan: Economic Development Institute, University of Ibadan. [RR ☆]
- . 1966. A plea for indigenous economics. *Economic Development and Cultural Change* 15:10-20.
- HOBBSAWM, ERIC. 1964. "Introduction," in *Pre-capitalist economic formations*, by Karl Marx. London: Lawrence and Wishart. [RF ☆]
- HOLMBERG, ALLAN R., et al., 1965. The changing values and institutions of Vicos in the context of national development. *American Behavioral Scientist* 18:3-8.
- HOMANS, GEORGE C. 1958. Social behaviour as exchange. *The American Journal of Sociology* 62:597-606.
- . 1964. Bringing men back in. *American Sociological Review* 29:808-18. [AS ☆]
- HUNTER, MONICA. 1961. Second edition. *Reaction to conquest*. London: Oxford University Press.
- JASPAN, M. A. 1953. "A sociological case study: Communal hostility to imposed social changes in South Africa," in *Approaches to community development*. Edited by Phillips Ruopp. The Hague: W. Van Hoeve.
- JOY, LEONARD. 1967a. "One economist's view of the relationship between economics and anthropology," in *Themes in economic anthropology*. Edited by R. Firth. London: Tavistock. [RF, HKS ☆]
- . 1967b. "An economic homologue of Barth's presentation of economic spheres in Darfur," in *Themes in economic anthropology*. Edited by R. Firth. London: Tavistock. [RF ☆]
- KAPLAN, DAVID. 1968. The formal-substantive controversy in economic anthropology:

Some reflections on its wider implications. *Southwestern Journal of Anthropology*. In press.

[HC☆]

- KEYFITZ, NATHAN. 1959. The interlocking of social and economic factors in Asian development. *The Canadian Journal of Economics and Political Science* 25:34-46.
- KEYNES, JOHN MAYNARD. 1936. *The general theory of employment, interest, and money*. New York: Harcourt, Brace.
- KNIGHT, FRANK. 1941. Anthropology and economics. *Journal of Political Economy* 49:247-68, (Reprinted in *Economic anthropology*, by M. J. Herskovits [New York: Knopf, 1952].)
- KOWALEWSKI, ZBIGNIEW MARCIN. 1967. Wspólnoty rolne chłopów andyjskiego Altiplano. *Łódzkie Studia Etnograficzne* 9. [ZMK☆]
- . 1968. Trzeci Świat: Rola wojny rewolucyjnej w organizowaniu kultury. *Łódzkie Studia Etnograficzne* 10. In press. [ZMK☆]
- KULA, W. 1962. *Teoria ekonomiczna ustroju feudalnego* (Economic theory of the feudal period). Warsaw. [ST☆]
- LEACH, E. R. 1960. "The Sinhalese of the Dry Zone of Northern Ceylon," in *Social structure in Southeast Asia*. Edited by G. P. Murdock. Chicago: Quadrangle Books. [HKS☆]
- LÊ CHÂU. 1966a. *Le Viet Nam socialiste: une économie de transition*. Paris: Maspero. [ZMK☆]
- . 1966b. *La révolution paysanne du Sud Viet Nam*. Paris: Maspero. [ZMK☆]
- LECLAIR, EDWARD E. 1962. Economic theory and economic anthropology. *American Anthropologist* 64:1179-1203.
- LECLAIR, EDWARD E., JR., and HAROLD K. SCHNEIDER. 1968. *Economic anthropology: Readings in theory and analysis*. New York: Holt, Rinehart and Winston. In press. [EEL☆]
- LÉVI-STRAUSS, CLAUDE. 1960. L'anthropologie sociale devant l'histoire. *Annales* 4. [ZMK☆]
- LEWIS, W. ARTHUR. 1955. *The theory of economic growth*. London: Allen and Unwin.
- . 1962. "Foreword," in *Economic development and social change in South India*, by T. Scarlett Epstein. Manchester: Manchester University Press.
- LIENHARDT, R. GODFREY. 1956. "Religion," in *Man, culture, and society*. Edited by Harry L. Shapiro. New York: Oxford University Press.
- MACIVER, R. M. 1933. *Society, its structure and changes*. New York: R. Long and R. R. Smith.
- MALINOWSKI, BRONISLAW. 1921. The primitive economics of the Trobriand islanders. *Economic Journal* 31:1-15.
- . 1922. *Argonauts of the western Pacific*. London: Routledge.
- . 1935. *Coral gardens and their magic*. Vol. 1. New York: American Book Company.
- . 1959. *Crime and custom in savage society*. Paterson: Littlefield, Adams. (First published in 1926.)
- MAO TSE TUNG. 1954. *How to analyse the classes in the rural areas*. Selected Works, vol. 1. London: Lawrence and Wishart. [RF☆]
- MAQUET, JACQUES. 1961. *The premise of inequality in Ruanda*. London: Oxford University Press.
- MARIATEGUI, JOSÉ CARLOS. 1963. *7 ensayos de interpretación de la realidad peruana*. Havana: Casa de las Américas. [ZMK☆]
- MARSHALL, ALFRED. 1920. *Principles of economics*. London: Macmillan.
- MARSHALL, GLORIA A. The growth and administration of rural markets in Western Nigeria. University of Michigan, Center for Research on Economic Development. MS. [JFR☆]
- MARTIN, KURT, and JOHN KNAPP. Editors. 1967. *The teaching of development economics*. Chicago: Aldine.
- MARX, KARL. 1953. "Einleitung zur Kritik der politischen Oekonomie," in *Grundrisse der Kritik der politischen Oekonomie*. Berlin: Dietz. [ZMK☆]
- . 1964. *Pre-capitalist economic formations*. London: Lawrence and Wishart. [RF☆]
- MASSELL, B. 1963. Econometric variations on a theme by Schneider. *Economic Development and Cultural Change* 12 (1):34-41. [HKS☆]
- MAUSS, MARCEL. 1954. *The gift: Forms and functions of exchange in archaic societies*. Glencoe: The Free Press.
- MOORE, BARRINGTON, JR. 1967. *Social origins of dictatorship and democracy: Lord and peasant in the making of the modern world*. Boston: Beacon. [JFR☆]
- MYRDAL, GUNNAR. 1957. *Rich lands and poor*. New York: Harper.
- . 1960. *Beyond the welfare state*. New Haven: Yale University Press.
- NADEL, S. F. 1942. *A black Byzantium: The kingdom of Nupe in Nigeria*. London: Oxford University Press.
- NASH, MANNING. 1966. *Primitive and peasant economic systems*. San Francisco: Chandler.
- . 1967. Reply to reviews of *Primitive and peasant economic systems*. *CURRENT ANTHROPOLOGY* 8:249-50.
- NEALE, WALTER C. 1957a. "Reciprocity and redistribution in the Indian village," in *Trade and market in the early empires*. Edited by K. Polanyi, C. M. Arensberg, and H. W. Pearson. Glencoe: The Free Press.
- . 1957b. "The market in theory and history," in *Trade and market in the early empires*. Edited by K. Polanyi, C. M. Arensberg, and H. W. Pearson. Glencoe: The Free Press.
- NORDHOFF, CHARLES. 1961. *The communistic societies of the United States*. New York: Hillary House. (First published in 1875.)
- NOVE, ALEC. 1962. *The Soviet economy*. New York: Praeger.
- NOYES, JOHN HUMPHREY. 1870. *American socialisms*. Philadelphia: J. B. Lippincott.
- NYERERE, JULIUS K. 1964. "Ujamaa," in *African socialism*. Edited by William H. Friedland and Carl G. Roseberg, Jr. Stanford: Stanford University Press.
- OKIGBO, PRUS. 1956. Social consequences of economic development in West Africa. *Annals of the American Academy of Political Science*: 125-33.
- PEARSON, HARRY W. 1957. "The economy has no surplus: Critique of a theory of development," in *Trade and market in the early empires*. Edited by K. Polanyi, C. M. Arensberg, and H. W. Pearson. Glencoe: The Free Press.
- PIRENNE, HENRI. 1936. *Economic and social history of medieval Europe*. London: Routledge and Kegan Paul.
- POLANYI, KARL. 1944. *The great transformation*. New York: Rinehart.
- . 1947. Our obsolete market mentality. *Commentary* 13:109-17.
- . 1957. "The economy as instituted process," in *Trade and market in the early empires*. Edited by K. Polanyi, C. M. Arensberg, and H. W. Pearson. Glencoe: The Free Press.
- . 1960. "On the comparative treatment of institutions in antiquity, with illustrations from Athens, Mycenae, and Alalakh," in *City invincible*. Edited by C. H. Kraeling and R. M. Adams. Chicago: University of Chicago Press.
- . 1963. Ports of trade in early societies. *Journal of Economic History* 23:30-45.
- . 1966. *Dahomey and the slave trade*. Seattle: University of Washington Press.
- . 1968. "The semantics of money uses," in *Primitive, archaic, and modern economies: Essays of Karl Polanyi*. Edited by G. Dalton. New York: Doubleday, Anchor Books.
- POSPISIL, LEOPOLD. 1963. *Kapauku Papuan economy*. Yale University Publications in Anthropology no. 67.
- POSTAN, M. M. 1966. *The agrarian life of the Middle Ages*. Volume I of *The Cambridge Economic History of Europe*, second edition. Cambridge: Cambridge University Press.
- POZAS, RICARDO. 1959. *Chamula*. México: Instituto Nacional Indigenista. [ZMK☆]
- REDFIELD, ROBERT. 1956. *Peasant society and culture*. Chicago: University of Chicago Press.
- REYNDERS, H. J. J. 1963. "The geographical income of the Bantu areas in South Africa," in *African studies in income and wealth*. Edited by L. H. Samuels. Chicago: Quadrangle Books.
- RICHARDS, AUDREY I. 1939. *Land, labor and diet in Northern Rhodesia*. London: Oxford University Press.
- RIVERS, W. H. R. 1922. *Essays on the depopulation of Melanesia*. Cambridge: University Press.
- ROBBINS, LIONEL. 1935. *An essay on the nature and significance of economic science*. London: Macmillan.
- ROBINSON, JOAN. 1932. *The economics of imperfect competition*. London: Macmillan.
- ROSTOW, W. W. Editor. 1963. *The economics of take-off into sustained growth*. London: Macmillan.
- ROTTENBERG, SIMON. 1958. Review of: *Trade and market in the early empires*, edited by K. Polanyi, C. M. Arensberg, and H. W. Pearson (Glencoe: The Free Press, 1957). *American Economic Review* 48:675-78.
- SAHLINS, MARSHALL D. 1958. *Social stratification in Polynesia*. American Ethnological Society Monograph. Seattle: University of Washington Press. [RF☆]
- . 1960. "Political power and the economy in primitive society," in *Essays in the science of culture in honor of Leslie White*. Edited by G. Dole and R. Carneiro. New York: Crowell.
- . 1962a. Review of *Sociological aspects of economic growth*, by B. F. Hoselitz (Glencoe: The Free Press, 1960). *American Anthropologist* 64:1063-73. [RF☆]
- . 1962b. *Moala*. Ann Arbor: University of Michigan Press. [RF☆]
- . 1963. Poor Man, Rich Man, Big-Man, Chief: Political types in Melanesia and Polynesia. *Comparative Studies in Society and History* 5:285-303. [RF☆]
- . 1965. "On the sociology of primitive exchange," in *The relevance of models for social anthropology*. Edited by M. Banton, pp. 139-236. A.S.A. Monograph no. 1. London: Tavistock. [RF☆]
- . 1968. "Tribal economics," in *Tribesmen*. Englewood Cliffs: Prentice-Hall.
- SALISBURY, R. F. 1962. *From stone to steel*. London and New York: Cambridge University Press.
- . 1966. Anthropology and economics. Paper presented at the annual meetings of the American Anthropological Society, Pittsburgh, November, 1966. (To appear in *Anthropology and the neighboring disciplines*. Edited by Otto von Mering. Pittsburgh: University of Pittsburgh Press. In press.) [RFS☆]
- SAWYER, JOHN E. 1951. Social structure and economic progress. *American Economic Review* 41:321-29.
- SCHAPERA, I. 1928. Economic changes in South African native life. *Africa* 1:170-88.
- . 1934. *Western civilization and the natives of South Africa*. London: Routledge.
- SCHAPERA, I., and A. J. H. GOODWIN. 1937. "Work and wealth," in *The Bantu-speaking tribes of South Africa*. Edited by I. Schapera. London: Routledge & Kegan Paul.
- SCHNEIDER, H. K. 1964a. "Economics in East African Aboriginal society," in *Economic transition in Africa*. Edited by M. J. Herskovits and M. Hartwitz. Evanston: Northwestern University Press. [HKS☆]



- . 1964b. A model of African indigenous economy and societies. *Comparative Studies in Society and History* 7:37-550. [HKS☆]
- SENGHOR, LEOPOLD S. 1964. *On African socialism*. New York: Praeger.
- SINGER, HANS. 1950. "The distribution of gains between borrowing and lending countries." *American Economic Review, Papers and Proceedings*, May, 1950.
- SINHA, D. P. 1967. The Phariya in an intertribal market. *Economic and Political Weekly* 2:1373-78. [DPS☆]
- . 1968. *Culture change in an intertribal market*. New York: Asia Publishing House. [DPS☆]
- SMELSER, NEIL J. 1958. *Social change in the industrial revolution*. London: Routledge & Kegan Paul.
- . 1963. "Mechanisms of change and adjustment to change," in *Industrialization and society*. Edited by B. F. Hoselitz and W. E. Moore. The Hague: UNESCO-Mouton.
- . Editor. 1965. *Readings on economic sociology*. Englewood Cliffs: Prentice-Hall. [RR☆]
- SOUTHALL, AIDAN. 1961. *Social change in modern Africa*. London: Oxford University Press.
- STAVENHAGEN, RODOLFO. 1963. Clases, colonialismo u aculturación: Ensayo sobre un sistema de relaciones interétnicas en Mesoamérica. *América Latina* 4. [ZMK☆]
- STEINER, FRANZ. 1957. Towards a classification of labor. *Sociologus* 7: 112-129
- SWEETZ, PAUL. Editor. *The transition from feudalism to capitalism*. London: Arena. [RF☆]
- TAKIZAWA, MATSUO. 1927. *The penetration of money economy in Japan and its effects upon social and political institutions*. New York: Columbia University Press.
- TAWNEY, R. H. 1967. *The agrarian problem in the sixteenth century*. New York: Harper and Row. (First published, London, 1912.) [JFR☆]
- TAX, SOL. 1963. *Penny capitalism*. Chicago: University of Chicago Press. (First published in 1953.)
- TORRES RESTREPO, CAMILO. 1966. La violencia y los cambios sociales. *La Gaceta* 16/17. [ZMK☆]
- UBEROI, J. P. SINGH. 1962. *The politics of the kula*. Manchester: Manchester University Press.
- UNITED NATIONS. 1954. *Enlargement of the exchange in tropical Africa*. New York: United Nations.
- UNESCO. 1963. *Social aspects of economic development in Latin America*. New York.
- VAYDA, ANDREW P. 1967. On the anthropological study of economies. *Journal of Economic Issues* 1:86-90. [APV☆]
- VON NEUMANN, J., and OSKAR MORGENSTERN. 1964. *Theory of games and economic behavior*. New York: John Wiley. [HKS☆]
- WALLACE, ANTHONY F. C. 1966. *Religion: An anthropological view*. New York: Random House. [EEL☆]
- WEBER, MAX. 1950. *General economic history*. Glencoe: The Free Press.
- WILSON, GODFREY. 1941. *An essay on the economics of detribalization in Northern Rhodesia*. The Rhodes-Livingstone Papers no. 5.
- WOLF, ERIC R. 1955. Types of Latin American peasantry. *American Anthropologist* 57 (3). [ZMK☆]
- . 1959. *Sons of the shaking earth*. Chicago: University of Chicago Press. [ZMK☆]
- . 1966. *Peasants*. Englewood Cliffs: Prentice-Hall.
- WOLFF, P. 1961. "L'étude des économies et des sociétés avant l'ère statistique," in *L'histoire et ses méthodes*. Edited by C. Samaran. Paris. [ST☆]
- WORSLEY, PETER. 1957a. *The trumpet shall sound: A study of "cargo" cults in Melanesia*. London: MacGibbon & Kee.
- . 1957b. Millenarian movements in Melanesia. *Rhodes-Livingstone Institute Journal*, pp. 18-31.
- . 1964a. *The third world*. London: Weidenfeld and Nicolson. [RF☆]
- . 1964b. "Bureaucracy and decolonisation Democracy from the top," in *The new sociology*. Edited by I. L. Horowitz. New York: Oxford University Press. [RF☆]
- YAMEY, B. S. 1964. "The study of peasant economic systems: Some concluding comments and questions," in *Capital, saving and credit in peasant societies*. Edited by Raymond Firth and B. S. Yamey, pp. 376-86. Chicago: Aldine. [RR☆]
- YANG, MARTIN C. 1945. "The family as a primary economic group," in *A Chinese village*. New York: Columbia University Press.
- YENGOYAN, A. 1966. Ecological analysis and traditional agriculture. *Comparative Studies in Society and History* 9:105-16. [JFR☆]
- YUDELMAN, MONTAGUE. 1964. *Africans on the land*. Cambridge: Harvard University Press.